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ART. XX.—*On the Phosphorograph of a Solar Spectrum, and on the Lines of its Infra-red Region;** by JOHN WILLIAM DRAPER, M.D., Professor of Chemistry in the University of New York.

I PROPOSE in this communication to consider: 1. The peculiarities of a phosphorograph of the solar spectrum as compared with a photograph of the same object; 2. The antagonization of effect of rays of higher by those of lower refrangibility.

There is a striking resemblance between a photograph of that spectrum taken on iodide of silver and a phosphorograph taken on luminous paint, and other phosphorescent preparations. There are also differences.

I. DESCRIPTION OF THE PHOTOGRAPHIC SPECTRUM.

In 1842, I obtained some very fine impressions of the first kind (on iodide of silver), and described them in the "Philosophical Magazine" (November, 1842), and again in February, 1847. One of these was made the subject of an elaborate examination by Sir J. Herschel. His description and explanatory views of it may be found in that Journal, February, 1843.

From these it appears that such a photograph, *taken in presence of a weak extraneous light*, may be considered as presenting three regions. 1. A middle one extending from the boundary of the blue and green to a little beyond the violet; in this region the argentic iodide is blackened. 2. Below this, and

* From the Proceedings of the American Academy of Arts and Sciences.

extending from the boundary of the blue and green to the inferior theoretical limit of the prismatic spectrum, is a region strongly marked in which the action of the daylight has been altogether arrested or removed, the daylight and the sunlight having apparently counterbalanced and checked each other. 3. A similar protected region occurs beyond the violet. This, however, is very much shorter than the preceding. The sketch annexed to Herschel's paper represents these facts as well as they can be represented by an uncolored drawing.

II. Description of the Phosphorographic Spectrum.

In a phosphorograph on luminous paint the same general effects appear. If the impression of the spectrum be taken in the absence of extraneous light, there is a shining region corresponding to the blackened region of the photograph. But if, previously or simultaneously, extraneous light be permitted to be present, new effects appear. The shining region of the phosphorograph has annexed to it, in the direction of the less refrangible spaces and extending toward the theoretical limit of the spectrum, a region of blackness in striking contrast to the surrounding luminous surface. The blackness is, however, broken at a distance below the red by a luminous rectangle of considerable width. This occupies the space, and indeed arises from the coalescence of the bands, α, β, γ, discovered by me in 1842. It may be separated into its constituent bands, which are very discernible when registered on gelatine as presently described. And since this is not so easily done with the upper lines of the spectrum, we may infer that these are very much broader than the Fraunhofer lines, a result strengthened by the fact that these dark intervals can be more easily recognized by the thermopile than those lines. The blackness is then resumed. It extends to a short distance, and there the phosphorographic impression comes to an end.

This shining rectangle has long been known to students of phosphorescence, but its interesting origin has not until now been explained.

But more, just beyond the region of the violet, the same kind of action occurs,—a dark space, which, however, is of very much less extent than that beyond the red.

The photograph and the phosphorograph thus present many points of similarity. But though there are these striking points of resemblance, there are also striking differences.

In a spectrum four or five centimeters long, though the photograph may be crossed by hundreds of Fraunhofer lines, not one is seen in the phosphorograph, except those just referred to. The spectrum must be dispersed much more before they can be discerned.

III. Of the Propagation of Phosphorescence from Particle to Particle.

The explanation of this disappearance of the Fraunhofer lines is obvious. A phosphorescing particle may emit light enough to cause others in its neighborhood to shine, and each of these in its turn may excite others, and so the luminosity may spread. In a former memoir I examined this in the case of chlorophane, and concluded that in that substance such a communication does not take place. But now, using more sensitive preparations, as follows, I have established in a satisfactory manner that it does.

The test plate referred to in the next paragraph was thus made. A piece of glass was smoked on one side in a flame, until it became quite opaque. When cool a few letters or words were written on it. Some photographic varnish was poured on it and drained. This, drying quickly, gave a black surface which could be handled without injury.

A phosphorographic tablet was made to shine by exposure to the sky. It was then carried into a dark room, and the test plate laid upon it. On the test plate another non-shining phosphorographic tablet was laid, and kept in that position a few minutes; then, on lifting this from the test plate, the letters were plainly visible, especially if it were laid on a piece of hot metal. So the light radiating from the first tablet through the letters of the test could produce phosphorescence in the second tablet, through glass more than a millimeter thick.

This lateral illumination is therefore sufficient to destroy the impression that is left by the fixed lines, unless indeed their breadth be sufficiently exaggerated, and as short an interval as possible permitted between the moment of insolation and that of observation.

It has been remarked that a photograph taken from a phosphorograph is never sharp. It looks as if it were taken out of focus, and this even though it may be a copy by contact. The light has spread from particle to particle. Under such circumstances, sharpness is impossible, because the phosphorograph itself is not sharp.

For this reason, also, the bright rectangle in a phosphorograph of the solar spectrum, arising from the coalescence of the infra-red lines α, β, γ, is never sharp on its edges. It seems as if it were fading away on either side. It is also broader than would correspond to the actual position and width of those lines, and, particularly, it is somewhat rounded at its corners.

If we could obtain a thermograph of the solar spectrum, it would correspond very closely to the phosphorograph. The particles heated would radiate their heat to adjacent ones.

Nothing like sharpness of definition could be obtained except in very brief exposures before the effect had had time to spread.

IV. Examination of Phosphorescent Tablets by Gelatine Photography.

The examination of a phosphorescent surface can be made now in a much more satisfactory manner than formerly. The light we have to deal with, being variable, declines from the moment of excitation to the moment of observation. And, though the phosphori now prepared are much more sensitive and persistent than those formerly made, they must still be looked upon as ephemeral. To examine them properly, the eye must have been a long time in darkness to acquire full sensitiveness.

It was recommended by Dufay to place a bandage over one eye that its sensitiveness might not be disturbed, whilst the other being left naked could be used in making the necessary preparations. But this on trial will be found, though occasionally useful, on the whole an uncomfortable and unsatisfactory method.

The exceedingly sensitive gelatine plates now obtainable remove these difficulties. The light emitted by blue phosphori, such as luminous paint, consists largely of rays between H and G, and these are rays which act at a maximum on the gelatine preparation. So if a gelatine plate be laid on a shining blue phosphorus it is powerfully affected, and any mark or image that may have been impressed on the phosphorus will on development in any of the usual ways be found on the gelatine. The gelatine has no need to wait after the manner of the eye. It sees the phosphorus instantly. It is impressed from the very first moment, and while the eye is accommodating itself and so losing the best of the effect, the gelatine is gathering every ray and losing nothing. Moreover, the effect upon it is cumulative. The eye is affected by the intensity of the emitted light, the gelatine by its quantity. Each moment adds to the effect of the preceding. The gelatine absorbs all the light that the phosphorus emits from the moment of excitation, or by suitable arrangement any fractional part thereof. It has another most important advantage. The phosphorus is yielding an ephemeral result, and is momentarily hastening to extinction, so that for a comparison of such a result with others of a like kind the memory must be trusted to. But the gelatine seizes it at any predetermined instant and keeps it forever. These permanent representations can at any future time be deliberately compared with one another.

To these still another advantage may be added. Very frequently an impression is much more perceptible on a gelatine copy than it is on the phosphorus from which that copy was taken. This arises from the fact that the eye is made less sensitive by the light emitted from surrounding phosphorescent parts, and cannot perceive a sombre point or line among them. That is a physiological effect. But a gelatine copy in no respect dazzles or enfeebles the eye. For this reason, for instance, we may not be able in a phosphorograph to resolve visually the infra-red bright rectangle into its constituent lines, but we recognize them instantly in the gelatine.

I have made use of sensitive gelatine plates ever since their quality of being affected by phosphorescent light was announced by Messrs. Warnecke and Darwin. The more sensitive of these plates receives a full effect by an exposure of less than one minute.

But all kinds of phosphori will not thus affect a photographic tablet: there must be a sympathy between the phosphorescent and the photographic surfaces. Thus a phosphorus emitting a yellow light will not affect a photographic preparation which requires blue or indigo rays. This principle I detected many years ago. In my memoir on phosphorescence (Phil. Mag., February, 1851), it will be seen that the green light emitted by chlorophane could not change the most sensitive photographic preparation at that time known—the daguerreotype plate—and hence I was obliged, in measuring the light it emits, to resort to Bouguer's optical method. The result would have turned out differently had the light to be measured been more refrangible, blue or indigo or violet.

A photographic surface agrees with the retina in this, that it has limits of sensitiveness. The eye is insensible to rays of much lower refrangibility than A, and much higher than H. Gelatine cannot perceive rays lower than F, but it is affected by others far higher than H. There is therefore a range for each, having its limits and also its place or point of maximum sensitiveness. But some substances, such as the iodide and bromoiodide of silver, under special methods of treatment are affected either positively or negatively throughout the entire range of the spectrum.

In experiments for obtaining quantitative results, it should be borne in mind that there is generally a loss of effect. Between the moment of insolation and that of perception, either by the eye or by gelatine, emitted light escapes. The moment of maximum emission is the moment of completed insolation, and from this the light rapidly declines. It is necessary, therefore, to make that interval between the two moments as short as possible.

V. Of the Extinction of Phosphorescence by Red Light.

I turn now to an examination of those parts of the phosphorographic spectrum from which the light has been removed. They are from the line F to the end of the infra-red space, and again for a short distance above the violet. The effect resembles the protecting action in the same region of a photograph.

Now, if similar effects are to be attributed to similar causes, we should expect to find in the photograph and phosphorograph the manifestation of a common action.

Several different explanations of the facts have been offered. Herschel suggested that the photograph might be interpreted on the optical principle of the colors of thin films. Very recently Captain Abney has attributed the appearance of the lower space to oxidation. But this can scarcely be the case in all instances. Mr. Claudet showed, in a very interesting paper on the action of red light, that a daguerreotype plate can be used again and again by the aid of a red glass, and that the sensitive film undergoes no chemical change. (Phil. Mag., February, 1848.)

It was known to the earliest experimenters on the subject that if the temperature of a phosphorescent surface be raised, the liberation of its light is hastened, and it more quickly relapses into the dark condition. In the memoir to which I have previously referred (Phil. Mag., February, 1851), I examined minutely into this effect of heat, and determined the conditions which regulate it. And since, on the old view of the constitution of the solar spectrum, the heat was supposed to increase toward the red ray, and when flint-glass or rock-salt prisms are employed to give its maximum far beyond that ray, it was supposed that this heat expelled the light, and consequently in all those parts of the phosphorus on which it fell the surface became dark through the expulsion or exhaustion of the light.

I speak of this as "the old view," because, as I have elsewhere shown, the curves supposed to represent heat, light, and actinism so called, have in reality nothing to do with those principles. They are merely dispersion curves having relation to the optical action of the prism and to the character of the surface on which the ray falls. (Phil. Mag., August, 1872, December, 1872.)

But this heat explanation of the phosphorescent facts cannot be applied to the photographic. Nothing in the way of hastened or secondary radiation seems to take place in that case.

In phosphorescence the facts observed in the production of this blackness are these. If a shining phosphorescent surface

be caused suddenly to receive a solar spectrum, it will instantly become brighter in the region of the less refrangible rays, as will plainly appear on the spectrum being for a moment extinguished by shutting off the light that comes into the dark room to form it. If the light be re-admitted again and again, the like increase of brilliancy may again and again be observed, but in a declining way. Presently, however, the region that has thus emitted its light begins to turn darker than the surrounding luminous parts. If now we no longer admit any spectrum light, but watch the phosphorescent surface as its luminosity slowly declines, the region that has thus shot forth its radiation becomes darker and darker, and at a certain time quite black. The surrounding parts in the course of some hours slowly overtake it, emitting the same quantity of light that had previously been expelled from it, and eventually all becomes dark.

Now, apparently, all this is in accordance with the hypothesis of the expulsion of the light by heat. There are, however, certain other facts which throw doubt on the correctness of that explanation.

On that hypothesis, the darkening ought to begin at the place of maximum heat, that is, when flint glass apparatus is used, below the red ray, and from this it should become less and less intense in the more refrangible direction. But, in many experiments carefully made, I have found that the maximum of blackness has its place of origin above the line D, and indeed where the orange and green rays touch each other. Not infrequently, in certain experiments the exact conditions of which I do not know and cannot always reproduce, the darkening begins at the upper confines of the green, and slowly passes down to beyond the red extremity; that is to say, its propagation is in the opposite direction to that which it ought to show on the heat hypothesis.

Still more, as has been stated, there is a dark space above the violet. Now it is commonly held that in this region there is little or no heat. If so, what is it that has expelled or destroyed the light?

The experiments above referred to I made with the recently introduced luminous paint. It presented the facts under their simplest form. But I have also tried many other samples, for which I am indebted to the courtesy of Professor Barker of Philadelphia. Among them I may mention as being very well known the specimens made by Dubosc, enclosed in flat glass tubes, contained in a mahogany case, and designed for illustrating the different colored phosphorescent lights emitted. They are to be found in most physical cabinets. These, however, do not show the facts in so clear a manner. On receiving the

impress of a solar spectrum they present patches of light and shade irregularly distributed. Though in a general way they confirm the statements made above, they do not do it sharply or satisfactorily.

Dubosc's specimens to which I have had access are enumerated as follows: 1. Calcium violet; 2. Calcium blue; 3. Calcium green; 4. Strontium green; 5. Strontium yellow; 6. Calcium orange. Restricting my observation to the space beyond the red,—which, as has been said, presents a bright rectangle in the darkness, about as far below the red as the red is below the yellow,—I found that this rectangle is not given by 1 and 2. In 3 it is doubtful. In 4 it is quite visible, and in 5 and 6 strikingly so.

Is the blackening then due to heat? That it occurs beyond the violet, that is, beyond the lines H, seems to render such an opinion doubtful, for it is commonly thought that the effect of heat is not recognizable there. And in the phosphorographic spectroscope I have used, the optical train, prism, lenses, etc., is of glass, which must of course exercise a special selective heat-absorption; but the traces of this in the phosphorograph I could never detect.

In the diffraction spectrum, I had attempted nearly forty years ago to ascertain the distribution of heat (Phil. Mag., March, 1857), but could not succeed with the experiment in a completely satisfactory manner, so small is the effect. I exposed a tablet of luminous paint to such a diffraction spectrum formed by a reflecting grating having 17,296 lines to the inch, and was not a little surprised to see that from the blue to the red end of the spectrum there is an energetic extinction of the light, and darkness is produced. I repeated this with other gratings, and under varied circumstances, and always found the same effect.

Now, considering the exceedingly small amount of heat available in this case, and considering the intensity of the effect, is there not herein an indication that we must attribute this result to some other than a calorific cause?

I endeavored to obtain better information on this point by using the rays of the moon, which, as is well known, are very deficient in heating power. Many years ago I had obtained some phosphorographs of that object. With the more sensitive preparations now accessible, and with a telescope 11 inches in aperture and 150 inches focus, there was no difficulty in procuring specimens about 1·4 inch in diameter. These represented the lunar surface satisfactorily. At half-moon an exposure of three or four seconds was sufficient to give a fair proof. But, on insolating a phosphorescent tablet, and causing the converging moon rays to pass through the red glass which I

commonly use as an extinguisher, no effect was produced by the red moonlight on the shining surface.

I repeated this experiment using a lens 5 inches in diameter and 7 inches focus so arranged that the moon's image could be kept stationary on the phosphorescent tablet. That image was about $\frac{1}{8}$ inch in diameter. Then, insolating the tablet, the moon rays, after passing through a red glass, were caused to fall upon it. The exposure continued ten minutes, but no effect was produced on the shining surface. The lunar image was so brilliant that when the red glass was removed, and a non-shining phosphorescent surface was exposed to it, a bright image could be produced in a single second.

But in order to remove the effect of the more refrangible rays by the less, the latter must not only have the proper wave length but also the proper amplitude of vibration. This principle applies both to photographic and phosphorographic experiments. In my memoir on the negative or protecting rays of the sun (Phil. Mag., February, 1847) it is said, "Before a perfect neutralization of action between two rays ensues, those rays must be adjusted in intensity to each other." It requires a powerful yellow ray to antagonize a feeble daylight.

It is owing to the difference in amplitude of vibration that the heat of radiation seems so much more effective than the heat of conduction. A temperature answering to that of the boiling point of mercury must be applied to a phosphorescent tablet for quite a considerable time before all the light is extinguished. But the red end of the spectrum and that even of the diffraction spectrum, in which the heat can with difficulty be detected by the most sensitive thermometer, accomplishes it very quickly.

VI. Of the Infra-red Lines or Bands in the Sun's Spectrum.

At a distance about as far below the red as the red is below the yellow in the solar spectrum, I found in 1842, in photographs taken on iodide of silver (Daguerre's preparation), three great lines or bands, with doubtful indications of a fourth still further off. I designated them as α, β, γ, and published an engraving of them in the Philosophical Magazine for May, 1843.

In 1846, MM. Foucault and Fizeau having repeated the experiment, thus originally made by me, presented a communication to the French Academy of Sciences. They had observed the antagonizing action above referred to, and had seen the infra-spectral lines α, β, γ. They had taken the precaution to deposit with the Academy a sealed envelope, containing an

account of their discovery, not knowing that it had been made and published long previously in America.

Sir J. Herschel had made some investigations on the distribution of heat in the spectrum, using paper blackened on one side and moistened with alcohol on the other. He obtained a series of spots or patches, commencing above the yellow and extending beyond the red. Some writers on this subject have considered that these observations imply a discovery of the lines α, β, γ. They forget, however, that Herschel did not use a slit, but the image of the Sun,—an image which was more than a quarter of an inch in diameter. Under such circumstances, it was impossible that these or any other of the fixed lines could be seen.

I have many times repeated this experiment, but could not obtain the same result, and therefore attributed my want of success to unskillfulness. More recently Lord Rayleigh (Phil. Mag., November, 1877), having experimented in the same direction, seems to be disposed to attribute these images to a misleading action of the prism employed. Whatever their cause may be, it is clear that they have nothing to do with the fixed lines α, β, γ, now under consideration.

In these experiments, and also in others made about the same time on the distribution of heat in the spectrum, I attempted to form a diffraction spectrum without the use of any dioptric media, endeavoring to get rid of all the disturbances which arise through the absorptive action of glass by using as the grating a polished surface of steel on which lines had been ruled with a diamond, and employing a concave mirror instead of an achromatic lens; and, though my results were imperfect and incomplete, I saw enough to convince me that it is absolutely necessary to employ a spectrum that has been formed by reflection alone. (Phil. Mag., March, 1857, p. 155.)

In 1871, M. Lamanski succeeded in detecting these lines or bands by the aid of a thermomultiplier. He was not adequately informed on what had already been done in the matter in America, for he says that "with the exception of Foucault and Fizeau, in their well-known experiments on the interference of heat, no one as yet has made reference to these lines." Nearly thirty years before the date of his memoir I had published an engraving of them. (Phil. Mag., May, 1843.)

After I had discovered these three lines, I intended to use the grating for the exploration of that region, since it extends it, far more than the prism can do; but, on making the attempt, was discouraged by the difficulty of getting rid of the more refrangible lines belonging to the second spectrum. I had hoped to eliminate these by passing the ray on its approach to the slit through a solution of the bichromate of potash. But

the bichromate in long exposures permits a sufficiency of the more refrangible rays to pass, to produce a marked photographic effect; and hence I feared that any experiments supposed to prove the existence of lines in the infra-red would be open to the criticism that they, in reality, belonged to the more refrangible regions of the spectrum of the second order, and that a satisfactory examination of the case would exclude the use of the grating and compel that of the prism. With the prism I could not obtain clear evidence of the existence of more than three lines, or perhaps groups, and doubtful indications of a fourth. If in these examinations we go as far as wave length 10,750, the limit of Captain Abney's map, we nearly reach the line H^3 of the third spectrum. This would include all the innumerable lines of spectrum 2, and even many of those of spectrum 3. In such a vast multitude of lines, how would it be possible to identify those that properly belonged to the first, and exclude those of the second and third spectra? Besides, do we not encounter the objection that this is altogether beyond the theoretical limit of the prismatic spectrum?

This brings us to Captain Abney's recent researches, which, by the aid of the grating, carry the investigation referred to the prismatic spectrum as far below the red as the red is below the yellow. They are not to be regarded as an extension of exploration in the infra-red region,—for they really do not carry us beyond my own observations in 1842,—but as securing the resolution of these lines or bands into their constituent elements. I had never regarded them as really single lines. The breadth or massiveness of their photographs, too, plainly suggests that they are composed of many associated ones. The principle of decreasing refrangibility with increasing wave length incapacitates the prism from separating them, but the grating which spreads them out according to their wave length reveals at once their composite character.

In Captain Abney's map, after leaving the red line A, we find three groups: (1) ranging from about 8150 to 8350; (2) from 8930 to 9300; (3) from 9350 to 9800. These, admitting that the lines of the subsequent grating spectra have been excluded, are then the resolution of α, β, γ.

I suppose that care has been taken to make sure of that, either by absorbent media or by a subsidiary prism. If the grating had been ruled in such a manner as to extinguish the second spectrum, inconveniences would arise from the characteristics thereby impressed on the first.

In the phosphorographic spectrum on luminous paint, this vast multitude of lines is blended into a mass which probably can never be completely resolved into its elements, on account of the propagation of phosphorescence from particle to particle. I have resolved it into two or three constituent groups, and fre-

quently have seen indications of its capability of resolution into lines, in the serrated aspect of its lateral edges.

I believe that luminous paint enables us to approach very nearly, if not completely, to the theoretical limit of the prismatic spectrum.

The history of these interesting infra-red lines is briefly this. They were discovered by me in 1842, and an engraving and description of them given in the "Philosophical Magazine." They were next seen by Foucault and Fizeau in 1846, and a description of them presented to the French Academy of Sciences. They were again detected by Lamanski with the thermopile in 1871. Their resolution into a great number of finer lines was accomplished by Abney, who gave a Bakerian lecture describing them before the Royal Society in 1880. Finally, they have been re-detected by me in the shining rectangle, just above the theoretical limit of the prismatic spectrum, given by many phosphorescent substances.

University of New York, Dec. 1, 1880.

ART. XXI.—*The structure and affinities of* Euphoberia *Meek and Worthen, a genus of Carboniferous Myriapoda;* by SAMUEL H. SCUDDER.

THE genus *Euphoberia* was established in 1868, for some remarkable spiny Myriapoda found in the ironstone nodules of Mazon Creek in Illinois, and which were first fully described and figured in the third volume of the Geological Report of the Illinois Survey. The only characteristics then noted, in which they differ from modern types, were the tapering form of the body, and the presence of branching spines on all the segments in longitudinal rows. An opportunity of examining a series of these animals from the same locality, due to the kindness of Messrs. Carr and Worthen, and especially of studying a fine fragment of *Euphoberia major* M. & W., giving an admirable view of the ventral plates, proves that the differences between these ancient types and modern forms are so numerous and important, as to compel us to refer them to a distinct suborder, for which the name of Archipolypoda is proposed.

One main distinction between the two groups, Diplopoda (or Chilognatha) and Chilopoda, into which existing Myriapoda are generally divided, consists in the relation of the ventral to the dorsal plates of the various segments of which the body is composed. In the Chilopoda there is a single ventral plate, bearing one pair of legs, for every dorsal plate: in the Diplopoda on the contrary, there are two such ventral plates, each bearing a pair of legs, for every dorsal plate (with the exception of a few segments at the extremities of the body).

The Diplopoda are universally considered the lower of the two in their organization, and it is therefore not surprising to find that no Chilopoda have been found in rocks older than the Tertiary series;* while Myriapods with two pair of legs corresponding to each dorsal plate may be found as far back as the Coal-measures. In such comparisons as are here instituted, the Chilopoda may therefore be left out of account.

In modern Diplopoda, each segment of the body is almost entirely composed of the dorsal plate, forming a nearly complete ring, for it encircles, as a general rule, nine-tenths of the body, leaving small room for the pair of ventral plates. On the side of the body it is perforated by a minute foramen, the opening of an odoriferous gland. Usually the ring is nearly circular, but occasionally the body is considerably flattened, and the sides are sometimes expanded into flattened laminæ with a smooth or serrate margin; a few species are provided with minute hairs, sometimes perched on little papillæ; and the surface of the body, ordinarily smooth or at best wrinkled, is occasionally beset with roughened tubercles, which may even form jagged projections. So far as I am aware, no nearer approach to spines occurs on this dorsal plate than the serrate edges of the lateral laminæ, the roughened tubercles or the papilla-mounted hairs.

In the Euphoberiæ from the Coal-measures a very different condition of things obtains. The segments of the body may be circular, or laterally compressed, or as in many modern types, depressed; but in all, the dorsal plate occupies scarcely more than two-thirds of the circuit of the body, or even less, being opposed by broad ventral plates. This dorsal plate is not perforated for foramina repugnatoria,† but, as means of defence, it is armed with two or three huge spines upon either side; one row (for they occur on all the segments alike) lies above, near the middle line of the body; another is placed low down upon the sides, near the lower margin of the dorsal plate; and a third row is sometimes found between them. These spines are sometimes forked at the tip, and they are (probably) always provided to a greater or less extent with spinules, springing from the base or the stem; sometimes these are so numerous as to form a *whorl* of little spines around the main stem; usually the main spines are at least half as long as the diameter of the body; often they are as long as the diameter, and one may readily picture the different appearance between one of these creatures, perhaps a foot or more in length,

* *Geophilus proavus* Germ., from the Jura, is certainly a nereid worm, as stated by Hagen.

† This is what would be expected from the presence of spines, for two such means of defence should not be looked for in the same animal; offensive glands are present only in slow-moving or otherwise defenceless creatures, as in Phasmidæ among Orthoptera for example.

bristling all over with a coarse tangle of thorny spines, and the smooth galley-worm of the present day.

If we pass to the ventral plates we shall find differences of even greater significance. In modern Diplopoda these plates are minute; the anterior forms the anterior edge of the segment, continuous with that of the dorsal plate; together, however, they are not so long as the dorsal plate at their side, and the latter appears partly to encircle the posterior of the ventral plates by extending inward toward the coxal cavities. The legs are attached to the posterior edge of each ventral plate, and those of opposite sides are so closely crowded together that they absolutely touch. The stigmata, of which there is a pair to each ventral plate, are placed at the outer edge, rather toward the front margin, and their openings are longitudinal, i. e., they lie athwart the segment; the coxæ of the legs of the anterior plate are therefore opposite the stigmata of the posterior plate. No other organs are found upon the ventral plates; one might indeed say there was not room for them. The legs themselves are composed of six simple, cylindrical joints, subequal in length, the apical armed with a single terminal claw; the whole leg is short, generally not more than half as long as the diameter of the body.

In the ancient Euphoberiæ, all is very different. The ventral plates occupy the entire ventral surface, perhaps may be said to extend partly up the sides of the rounded body, and no part of the dorsal plate passes behind the posterior ventral plate; they are together equal in length to any part of the dorsal plate, the segments of the body being equal in length throughout; while in modern Diplopoda the upper portion of the dorsal plate is always considerably longer than the ventral portion, allowing the creature to coil ventrally without exposing any intersegmental portion of the back devoid of hard armature; while in these ancient forms, the animal appears to coil dorsally as readily as ventrally; at least, when not extended straight upon the stones in which they are preserved, they are as frequently found bent upward as downward; and there is certainly nothing in their structure to prevent such mobility.

Then the legs, instead of being inserted at the extreme posterior edge of the plate, are planted almost in its very center, and are indeed so large that they occupy nearly its entire width; neither are those of opposite sides inserted close together, but are removed from one another by a space equal to their own width, giving them ample play. The legs themselves differ from those of modern types in having the second joint as long as the others combined, and the whole leg at least as long as the diameter of the body, and sometimes nearly twice as long; moreover they are not cylindrical but compressed and slightly expanded, strengthened also on the flattened surface by longi-

tudinal carinæ, and in every respect, in those specimens in which the legs are best preserved, have the aspect of *swimming organs*. No aquatic forms are known among recent myriapods.

The stigmata, instead of having the position they hold in modern Diplopoda, where they are necessarily minute, are very large, situated in the middle of each ventral plate, each spiracle opposite to and indeed touching the outside of the coxal cavity of the plate to which it belongs, and running therefore with and not athwart the plate, i. e., across the body. But in addition to these structures, which make up the sum of the furniture of the ventral plate in modern Diplopoda, we find in these ancient myriapods some further interesting organs, which are so perfectly preserved that no doubt can be entertained concerning their presence and their adherence to the ventral plate. The coxal cavities are not circular but oval and are situated with the major axis in an oblique line, running from near the middle line of the body forward and outward; this and the slight posterior insertion of the legs leave even a wider space between them at the anterior border of the plate than posteriorly, and this place is occupied by a pair of peculiar organs, situated one on either side of the median line at the very front edge of every ventral plate. These I think may be supports for branchiæ; they consist of little triangular cups or craters, projecting outward from the under surface, through which the branchial appendages protruded. Until recently no other organs than branchiæ have been found in any arthropod, situated within the legs, and repeated on segment after segment. The only exceptions known are Peripatus, a strange creature, allied certainly to the myriapods, but of lower organization, in which Balfour has found segmental organs (heretofore known only in worms) having their external openings somewhat similarly situated; and Scolopendrella, a minute chilopodous myriapod, in which Ryder has just described organs which he calls tracheæ, opening externally between the legs. But as branchiæ also occur *together with spiracles* in some low organized insects, and then in essentially similar relative positions to that in which they are here found; and as the possession of legs adapted to swimming leads us to presume in these creatures an aquatic or amphibious life: it would seem as if we might fairly conceive these crateriform appendages to be branchial supports,* and conclude that we are dealing with a type of myriapods very different from any existing forms,—suited to an amphibious life, capable of moving and breathing both on land and in water. Moreover the assemblage of forms discovered in the Mazon Creek beds lends force to this proposition; for the prevalence of aquatic Crustacea, of fishes and ferns, indicates that the fauna

* Even if they were segmental organs, they may still have been connected with respiration.

and flora was that of a region abounding in low and boggy land and pools; and the presence of marsh-frequenting flying insects does not contradict such a belief.

These, however, are not the only points in which the ancient forms differed from the recent. We have so far examined only a typical segment; let us now look at the body as a whole and at special segments. The modern Diplopoda are of uniform size throughout, tapering only at the extreme tips; while these ancient forms, at least when seen from above, diminish noticeably in size toward either end, and especially toward the tail, giving the body a fusiform appearance, its largest part being in the neighborhood of the seventh to the tenth body segments, which were often two, or even three, times broader than the hinder extremity, and considerably broader than the head or the first segment behind it. A single segment seems to have carried all the appendages related to the mouth parts, while in modern Diplopoda two segments are required for this purpose; this peculiarity of the fossil is inferred solely but sufficiently from the fact, perhaps even more remarkable, that every segment of the body (as represented by the dorsal plates), even those immediately following the single head-segment, is furnished with *two* ventral plates and bears *two* pair of legs; as is well known, each of the segments immediately following the head-segments in existing Diplopoda bears only one ventral plate, and only a single pair of legs,—a fact correlated with the embryonic growth of these creatures, since these legs and these only are first developed in the young diplopod. The mature forms of recent Diplopoda, therefore, resemble their own young more than do these Carboniferous myriapods, a fact which is certainly at variance with the general accord between ancient types and the embryonic condition of their modern representatives, and one for which we offer no explanatory suggestion worth consideration.

Unfortunately the preservation of the appendages of the head in these Carboniferous forms is not sufficiently good in any that have yet been found to allow any comparison with modern types. This is the more to be regretted since these parts are those on which we depend largely for our judgment of the relationship of the Myriapoda to other Insecta and to Crustacea. If they were present and sufficiently well defined, we may well suppose that they would afford some clue to the genetic connection of these great groups.

The structure of the Carboniferous Euphoberiæ have thus been shown to differ so much from that of modern Diplopoda that, as stated at the outset, we seem warranted in placing them in a group apart from either of the sub-orders of modern Myriapoda and of an equivalent taxonomic value.

Cambridge, January 7, 1881.

Art. XXII.—*The Actinic Balance;* by S. P. Langley.

The writer has been, during some time, making experiments on the device and construction of an instrument more delicate and more prompt than the thermopile; an advance which his recent researches into the distribution of radiant energy in the spectrum have proved to be indispensable. These researches have involved expenses for special apparatus which have been in part met by a grant from the American Academy of Arts and Sciences as trustees of Count Rumford, and the writer has with gratitude to acknowledge his past indebtedness to this aid. A communication of some of the principal results obtained was made to that society in the early part of December, and will appear with illustrations of the apparatus in a forthcoming volume of their Proceedings, to which the reader who desires fuller details is referred.

The following independent description of the newly devised apparatus is rendered necessary here, as an introduction to a future account of researches in the true distribution of radiant energy in the solar spectrum.

We see within the past few years a greatly increased attention to this subject and an attempt by many skilled observers to measure the distribution of heat for each individual ray with the minimum of error which the vicious method of the prism admits. Even the use of the prism, however, demands most delicate means of measurement. Tyndall, employing every instrumental aid science commanded in his experiments on the electric light, was obliged to operate on a spectrum only an inch and a half in length, and it is from this that the well-known heat curves of our text-books are derived. When we form a much longer spectrum, we must either make the face of our thermopile larger, or expect to find the radiation so weakened that we cannot measure it.

Now the use of the prismatic spectrum involves two prominent causes of error. One of these (well-known and partly guarded against) concerns the selective absorption of the material employed, the other, far more important and pernicious in its results, has been almost completely neglected. It concerns the fact that the prism acts a part analogous to that of a cylindrical lens, concentrating the rays in the lower part of the spectrum as compared with the upper and entirely falsifying in its specious results the true distribution of the heat. Even if there were no theoretical difficulty the measure of the refracted heat as distributed by the prism is far from easy on account of its feebleness, but there has seemed to be no choice,

for if the measurement of sensibly homogeneous rays of heat in the prismatic spectrum is difficult, the corresponding measurement in the diffraction spectrum is impossible, or has, at any rate until lately, been considered so. Under the most favorable circumstances, the total heat in the brightest spectrum formed by a diffraction grating is but about one-tenth of that in a prismatic spectrum of equal length, and this latter is itself, as we have just seen an all but vanishing quantity. Müller and others have attempted its evaluation without obtaining the least measurable effect, even from the aggregate heat of the entire spectrum. The elder Draper, baffled by the same difficulty, succeeded nevertheless in dividing the heat of the spectrum formed by a diffraction grating into two parts, which led him to announce, that the position of the maximum of heat did not differ greatly from that of light, a conclusion which has not apparently met the general attention it deserves from physicists thus far, at least in Europe.

In these most delicate measurements, the experimenter's great difficulty is to avoid the disturbing influence of extraneous sources of heat, which (since radiations of this latter class are almost wholly invisible), is extremely difficult, for we *see* nothing of the hundreds of insidious actions which are incessantly affecting our thermopile from sources that have nothing to do with the result we seek. There is probably no instrument in the whole range of scientific apparatus which demands a longer experience for its successful use than the thermopile, where we are employing it not for the purpose of a lecture experiment, but for the determination of some one almost infinitesimal radiation in the midst of numberless others which our only concern with, is to avoid. In fact the successful use of the thermopile, as employed by Melloni and by Tyndall may almost be assimilated to a kind of handicraft, requiring long familiarity and the almost instinctive and unconscious adoption at every moment of precautions which would certainly never suggest themselves to the untried observer. The writer, having served a long apprenticeship in previous years to this craft, had flattered himself that he might turn his familiarity with the thermopile to some useful account here, and might perhaps succeed from this cause, where others had failed. He was obliged, however, to admit to himself that his success was so partial as to be very like failure. He succeeded with the thermopile in obtaining feeble indications of heat on comparatively homogeneous rays in the diffraction reflection spectrum but these indications were all too feeble, and obtained at too great a cost of time and labor to make it possible to carry on our knowledge of the distribution of heat in the spectrum by means of the thermopile to any great extent beyond the point where others had left it.

These first experiments were made by letting the sun-light, which had passed through a distant slit, fall (without the intervention of a collimating lens), upon a grating, whence the diffracted rays fell upon a metallic mirror which concentrated them into a spectrum, pure enough to give even with a widely open slit all the leading Fraunhofer lines, but in this case, as has been said, the most delicate thermopiles procurable and the best galvanometer, then in the writer's possession, gave no results which could be relied on to anything like the desired degree of accuracy, and although something was gained it was too small an advance upon what had been done already, to seem worthy of publication. Satisfied that nothing more could be done here with the apparatus employed, and that the whole march of progress in this direction was arrested for want of a better instrument than science possessed, the writer attempted to find one. He commenced his investigations in November, 1879, and continued them almost without intermission until the autumn of 1880, when he found himself in possession of an instrument, not only greatly more sensitive than any thermopile, but also far more prompt, and as he believes more accurate.

The thermopile, it will be remembered, employs as the source of the force which swings the needle of the galvanometer, the feeble ray which falls upon the pile's face. There is no other force to move the needle than the excessively feeble energy locked up in the ray itself. It occurred to the writer (as it had occurred to others before him, doubtless), that he might use a feeble energy, not directly, but as the modulator of some greater force. The whole energy in one of the rays we are dealing with, for instance, will not give one-millionth part of the impulse upon a magnetic needle, which could be derived from a single Daniell's cell. But with the cell whose potential energy bears somewhat the same relation to that of the ray, that the strength of a steam-engine does to the strength of the human finger, we might make the feeble power of this ray play the same part that the finger would when laid on the engine's throttle-valve.

When (to use a common illustration), the finger is applied to the trigger of a gun, the little force liberates an indefinitely greater one, which has no certain relation to the energy of the original impulse. But what we here need is a rigorous *proportionality* between the feeble but momentarily varying energy of the original ray, and the amount of power it releases from a battery or other source of energy. It is only on these conditions that the indications of our instrument will be accordant and that it will be truly a meter. If we are in search only for extreme sensitiveness, and are satisfied to have a delicate ther-

moscope rather than an accurate thermometer, I believe that by employing some of the metalloids such as sulphur, selenium, or tellurium it is possible to make an instrument far more sensitive than that about to be described, but I repeat this sensitiveness is not all. What the working physicist wants is not an enormous result, bearing no definable relation to the original cause, but one which is strictly proportional to it. The useful instrument, then, is one which shall always give nearly the same results under repeated trials, or which shall in more technical language have a small probable error. I commenced, guided by these considerations, and with the aid of Mr. F. W. Very, in December, 1879, to experiment in the following direction. The *principle* has been employed by Jamin, by Siemens and by others, before. The following *application* is, I believe, new. Let us suppose that from a battery two wires of equal length and equal section pass to a differential galvanometer so that one current tends to move the needle to the right, the other and equal current, tends to move it to the left, and the needle solicited in opposite ways by equal forces, remains motionless at zero. Suppose now a ray from the sun, from a vessel of hot water, from a candle, or from any source, of radiant energy of higher temperature than the wires themselves, to fall on one of them; this wire becomes heated and therefore a worse conductor than before, and as its resistance increases in nearly the ratio of its increased temperature, there is less current through the heated wire, and the needle is deflected by a force which is strictly proportional in theory to the energy in the original ray, to the energy of the battery, and to certain constants of the galvanometer and the rest of the circuit. In what has just been said, it is temporarily assumed that all the energy of the original ray is represented by heat in the wire, and that none of it has been lost by conduction, convection, or by re-radiation. It is also supposed that no large change of temperature has taken place in the wire, but that the heating energy of the original ray is small. The last condition is only too easily met in practice. The consideration of the first will be resumed in another place.

We have just indicated merely the fundamental conception which is to guide our search for an actual instrument. Between this conception and the partial realization a great many months of assiduous and often disheartening labor have been expended, and complete success is far from having been reached even now, but as I believe it certain that the instrument in its actual stage of progress has been already successful in doing important work quite out of the thermopile's reach, I shall describe briefly the considerations which have led to its execution in its present form. Some of these are obvious. The

motion of the galvanometer needle will be proportional, other things being equal, to the change of resistance, and this change of resistance will be proportional to the actual resistance of the wire, as well as to the cross section of the ray falling upon it. If for instance we employ a naked copper wire one hundred meters in length, whose resistance is one ohm, the resistance of any given centimeter of its length will not exceed one ten-thousandth of an ohm, and accordingly, if from any source of radiant heat we let fall on the wire a ray which we will suppose to be one square centimeter in section, if it altered the resistance of the wire by so much as one one-hundredth part where it fell, we should have but one one-millionth of an ohm to produce the requisite change in the recording instrument. Evidently we must form the particular minute portion of the circuit on which the ray is to fall of some conductor which has a very high resistance indeed, as compared with the average resistance of the wires. If for instance we introduce a bit of gold-foil having a resistance of one ohm to the single centimeter so that it shall form a virtual portion of one of the wires, and if we let the ray fall on this, we now produce a change equal to the one one-hundredth part of one ohm, which is ten thousand times the effect produced before by the same cause. Similar considerations show us that the cylindrical form of the wire is a bad one, and that the metal used for the part exposed to radiant heat should be laminated, when it may be made to present a much greater surface to the source of radiant heat, with precisely the same conductibility. It is clear then that the following conditions should unite; great electric resistance; considerable change in resistance for a given change in heat, and a form which enables it to take up and part with heat very rapidly.

To this we may add among the minor conditions that it is desirable that the exposed portion should be of a metal readily reduced to thin laminæ, that it should be non-oxidizable, since it is to be in an excessively thin strip, and that it should also have sufficient rigidity to preserve its form. The whole of these conditions can rarely or never be found in the same substance. We must select our metal with a view to those conditions which are of most importance. Experiments were made in December, 1879, and in January and February, 1880, on a great variety of metals. They finally conducted me to the use of iron (or steel), platinum and palladium as the most available ones. Gold in the form of foil is unsuitable on account of minute rents made by the blows of the hammer. Metals on glass do not work as well on account of the heat taken up by the glass; but to attempt to narrate all the trials made would be useless. To comprehend the apparatus used, not in its ordi-

nary form, but in its most elementary type, let us suppose that we have succeeded in rolling steel until its thickness is $\frac{1}{3000}$ to $\frac{1}{3000}^{mm}$. In this state 8,000 to 12,000 sheets laid one on the other will make but about one English inch. It may be easily supposed that it is no light task to procure such a sheet of steel in the first instance. This, however, has been done successfully.

In order to fix our ideas let us now suppose two such pieces of steel, each rather less than $\frac{1}{2}$ inch long and $\frac{1}{20}$ inch wide, to be stretched side by side and almost in juxtaposition, within a small cylinder open at one end, which can be directed to the source of radiant heat, while the two strips are made to form each a portion of a circuit leading from the battery to the differential galvanometer. Since the change of resistance in iron is about $\frac{4}{100}$ of one per cent for each degree Centigrade, as minute a change of temperature as is represented by a single degree will cause a difference of resistance in the strips of $\frac{1}{2000}$. Thus supposing the resistance of the exposed part to be $\frac{1}{4}$ that of the whole circuit, there will be a differential effect upon the galvanometer equal to nearly $\frac{1}{10000}$ of the entire power of the battery, an enormous amount of force as compared with that represented by the heat required in warming such a minute mass as the strip by 1° C. Further, since the strip is so thin it will take up and part with the heat almost instantly. Where the thermopile often requires an exposure of five, ten or fifteen minutes, the strip will take up sensibly all of the heat it is capable of holding within a single second. There is here then a great gain in sensitiveness over the pile and also a great gain in rapidity. In the case of the pile, if we have a beam of heat of $\frac{1}{4}$ the cross-section, we employ to utilize it a pile of $\frac{1}{4}$ the size and get (roughly speaking) but $\frac{1}{4}$ the effect. With the strips we have the paradoxical result (in appearance at least) that $\frac{1}{4}$ the heat may produce the same effect as the whole. To see this more clearly, let us suppose another strip to be but $\frac{1}{2}$ as long and $\frac{1}{2}$ as wide as the first. This exposes but $\frac{1}{4}$ the area, but the resistance is obviously the same as before and any given percentage of that resistance will produce the same effect as before. From any thing which has been said, it might seem to follow then, that with a strip of the size of 1 square mm. we shall get the same effect as with one a meter square, or, in the last result that an infinitesimal source of heat will through this means produce the same effect as an infinitely great one under similar circumstances. This of course indicates a fallacy whose seat is found in our assumption that no change takes place in the temperature of the strip except from radiant heat. In reality it is not so. The current received from the battery itself of course produces heat in all parts of the circuit, and

this is particularly noticeable in the strips which offer large resistance in small compass. There is then a practical limit beyond which we cannot go without the battery heating of the strips becoming prejudicial, but the paradox we have alluded to is so near to fact, that it is found to be the case in actual practice that within certain limits the action of the strip is nearly independent of its size. Accordingly if we take ten contiguous parallel strips each one mm. wide and one cm. long and joining their alternate ends place them side by side (but not in contact) so as to present a total area of one cm. square, the effect on the galvanometer will be, i. e. approximately, ten times that of a single strip one cm. square carrying the same current, if that current be small. Of course, the single wide strip can carry a much greater current while being heated to no greater temperature than one of the narrow ones, and we thus see that we may choose between a feeble current, carried through numerous narrow strips with high local resistance, and a relatively strong current. The latter construction is far easier, but particular considerations have determined the actual trial of the former plan (of higher resistance and feeble currents) in the instruments employed.

In the latter case the current is led through all these strips in succession, and each of the two strips we have spoken of as existing in the simplest type of the instrument may then be very advantageously replaced in practice by a complete system of such strips. If the two systems are placed side by side, enclosed within non-conducting walls, they are evidently exposed, as far as possible, alike, to all changes of temperature arising from their immediate environment. When the containing cylinder is warmed or cooled, each system is warmed or cooled in an equal degree, and the galvanometer needle remains unmoved.

It is found in practice often more convenient not to use the differential galvanometer, but to make each system one of the arms of a Wheatstone's bridge. The little cylinder containing the strips is connected by insulated wires with a quite distant galvanometer, and this cylinder, containing the effective part of the apparatus, can be carried then by hand from one room to another, and so long as no radiant heat from a source directly in the prolongation of its axis enters it, the galvanometer needle remains unmoved; but if such a beam of radiant heat be allowed to enter and fall on one of the strips only, the instrument will instantly respond, while it remains unmoved by all accidental surrounding radiations. A still further improvement in the disposition of the strips is made by leading one of them in the center of the cylinder and by dividing the other into two equal parts, which are left one either side of the

central one, so as to be in identical circumstances of environment. The instrument therefore acts by any change in the equality of these two arms of the bridge in a manner quite analogous to the action of a chemist's balance, but with an extent of range which it is not probable that any chemist's balance can approach.

Under any circumstances, in view of the measurements we expect to make, a very delicate galvanometer of moderate (though not the lowest) resistance, will be a suitable instrument. The one used is of the most recent form of the Thomson galvanometer pattern. This instrument, just made by Elliott Bros., is more sensitive than any the writer has before used, and to its excellence he is no doubt in part indebted for the results attained.

It is perhaps well to introduce here the remark that nothing is gained by pushing the battery power beyond ordinary limits; both wires heat equally, as far as this battery current is concerned, but the air within the cylinder is no longer still, but fluctuates irregularly even when no radiant heat disturbs the "Actinic Balance," as the instrument just described will be provisionally called. As it is difficult to make an absolutely perfect balance within the strips themselves, a resistance box is commonly introduced in the circuit. The same battery power is used whether the resistance in the strip portion of the circuit be greater or less, but changes in the condition of the battery are kept from affecting the result by introducing an easily varied battery-shunt. The current employed is usually not enough to warm the strips, at the most, as much as 5° C. above the temperature of the environment, a condition best obtained with the strips actually used by an absolute current of less than $\frac{2}{10}$ Webers. Even with $\frac{1}{100}$ Webers we have a greater force at disposal than the excitant radiation here dealt with could ever develop in a thermopile.

It will be understood that experiments are still in progress with this instrument, but as it is doubtful when they will be concluded, and as it is certain it is capable of doing some things even in its present state, which the thermopile cannot, it seems proper to give the preceding description, without waiting for further improvement. It may be observed that the three metals which have hitherto been found best are steel, platinum and palladium. Steel fulfills every condition which may be desired save one. It is, unfortunately, oxidizable and is therefore difficult of preservation. Platinum may perhaps be found, upon the whole, more advantageous, though this is not yet certain. Palladium has hardly been experimented upon sufficiently to pronounce on with confidence, though it seems to promise well.

The instrument in its present condition has been used without any lamp-black upon the steel strips, for fear that its well-known hygrometric qualities might injure them by causing rust, but this objection will not apply to the platinum. The writer, however, has grave doubts about the advisability of treating the lamp-black as absorbing all heat-rays indifferently, although such a statement of its capacity is given by Melloni, and is very widely adopted on his eminent authority. A special investigation into the absorptive power of lamp-black will probably form a part of the present series of researches undertaken here. In its unblackened condition the instrument appears to be, roughly speaking, from 5 to 100 times as sensitive as the most sensitive thermopile the observer possesses, area for area. With the lowest degrees of sensitiveness its probable error is very small. The higher degrees, however, involve the pushing of the battery power to such an extent as to cause sensible heating of the wires and the consequent fluctuations on the galvanometer already alluded to, and increase of all accidental irregularities. The probable error will of course depend also on the condition of the galvanometer. If the source of heat is feeble, and the galvanometer adjusted to the condition of nearly complete astaticism (in other words of nearly complete instability), the probable error cannot fail to be considerable. If the source of heat is considerable and the needle's period of vibration brief, the probable error is of course less. These are general considerations, which affect alike the thermopile and actinic balance, but the latter instrument, putting a greater amount of power at our disposal, enables us to use the galvanometer needle in a much more stable condition, and for this and other causes enables readings to be obtained which are not only certainly far more rapid than the thermopile's, but apparently more exact. Under favorable circumstances, in repeating some of Melloni's experiments on radiation through distilled water and alum solutions with the unblackened steel balance, we have found the probable error of a *single* observation very much within one per cent. On the other hand, when working with an extremely faint source of heat, such for instance as the lunar rays give in a refracting telescope, the probable error of a single observation may be much larger.

A few observations are given here just as they are found in the note books, and which probably represent fairly the average accuracy of the instrument in its present condition, except that the source of heat being the sun, whose radiation varies from moment to moment from atmospheric causes, the probable error is larger than it would have been with a constant source of heat. The galvanometer used in the repetition of Melloni's

measures was an old and comparatively rough one, adjusted as nearly to *in*sensitiveness as possible by its directing magnet, so that the image should not be thrown off the scale. The actual readings are given of a series of seven measurements on the transmissibility of solar rays by water and by a solution of common alum containing ten per cent by weight, as taken by Mr. F. W. Very, on Sept. 22d, 1880, 11¼ A. M. to 1 P. M.

The liquids being enclosed in glass cells (sides $2{\cdot}5^{mm}$ thick, distance between sides $=19{\cdot}0^{mm}$) were interposed or withdrawn by sliding the stand, on which the cells were held perpendicular to a sunbeam, so that the center of the circular cell should fall opposite the aperture of the actinic balance case, which was inclined so as to point to the sun. Each reading on the sun is the mean of two taken directly before and after the interposition of the liquid.

Deflection by Radiation of full Sun.		Through Water.	Per cent Transmitted.	Deflection by Radiation of full Sun.		Through Alum sol.	Per cent Transmitted.
205 / 215	210	123	58·6	277 / 287	282	152	53·9
214 / 219	217	127	58·5	291 / 272	287	153	53·3
226 / 239	233	132	56·7	275 / 240	253	130	51·4
	253	149	58·9	283 / 283	283	150	53·0
	225	135	60·0	285 / 272	279	148	53·1
286 / 289	288	174	60·4	283 / 265	274	145	52·9
299 / 284	292	178	61·0	270 / 248	259	138	53·3

Solar radiation transmitted by water and glass = 59·2 per cent ± ·36.
Probable error of one observation = ± 0·96 per cent.
Solar radiation transmitted by alum and glass = 53·0 per cent ± ·19.
Probable error of one observation = ± 0·51 per cent.

These readings are given merely as fair samples of the average (not of the best) work where sensitiveness is not demanded.

As an instance of a by far more delicate class of measurements, we will use the following preliminary observations upon the heat of the moon, taken with another and more sensitive galvanometer, on the night of November 12th, 1880, with the equatorial of the observatory; employing its 13-inch achromatic lens and a smaller condensing lens near the focus. A great variety of precautions were used which are not here detailed, and the measurements were varied by taking them under as diverse conditions and in as many different ways as possible. Eighteen measures were taken in about an hour and a half.

All are given here exactly as they were obtained, except one, which involved an obvious error, but this one equally indicated heat.

		Actual deflections (heat).
Balance cover open. Balance exposed at all times to sky and lens radiation. Lunar image brought in by moving the telescope by hand.	West limb enters first.	53
		28
		53
		56
		44
		36
		70
	East limb enters first.	62
		39
		30
		50
Balance covered except during exposure and image kept in the same position at all times by the equatorial clock.		19
		26
		23
		41
		27
		51

Mean deflection = 41·6 ± 2·4. (Prob. error of a single observation = 9·9).

The preceding measures on lunar heat will doubtless be improved on. They are the first taken and are given here not for any intrinsic value of their own, but as aids in judging the capacity of the balance, to not merely indicate, but *measure* forms of radiant energy hitherto supposed beyond reach; for I am not aware of any previous authentic measures of the lunar rays which have passed through the *glass* lenses of a refracting telescope.

It may be mentioned that with this very feeble radiation the strips occupied no sensible time in heating, the galvanometer indications being smaller but as prompt and decisive as though the observations were on the sun.

A far more important practical result has been the measurement of nearly homogeneous rays of heat in the diffraction spectrum, which is now going on and of which an account will shortly be published. It may be stated briefly that with a diffraction spectrum so pure as to show all the principal lines on the screen, but in which the most delicate thermopile gives no satisfactory indications of heat, the actinic balance is found to determine the energy not only in the ultra red but in the ultra violet. The results promise to be of theoretical and practical interest. It may be said generally that they *experimentally* prove the absence of any maxima in the spectrum, such as are shown in the curves of "actinism," "light" and "heat," given in the text-books, and that they show that the actual solar energy (as it reaches us through our atmosphere), is nearly at its minimum precisely where the so-called "actinic" curve is

shown at its maximum. The special sensitiveness of certain salts of silver, then, to wave-lengths of from 0·0003^{mm} to 0·0004^{mm} and not any special solar energy or special modification of it, is the cause of the once-supposed peculiar "actinic power" of this part of the spectrum. *The maximum of "heat" is found to be not in the ultra red but in the orange, and the curve of "heat" is found to at least approximately agree with that of "light,"* by direct experiment, and other results of equal theoretical interest, and still more practical importance begin to appear.

The need to precede their statement in an early publication by some account of the means by which they were derived has been the occasion of this article.

Allegheny Observatory, Dec. 23, 1880.

ART. XXIII.—*Notices of Recent American Earthquakes.* No. 10; by Professor C. G. ROCKWOOD, Jr., Ph.D., Princeton, N. J.

THE present article embodies such information as has been obtained in regard to earthquakes occurring upon the American continent and adjacent islands since March 1, 1880; with notice of some earlier shocks not previously reported in this Journal. Items which depend upon single sources of information have their source indicated, and if regarded as at all doubtful are printed in smaller type.

In addition to the persons mentioned in the notices, the author would express his indebtedness to J. M. Batchelder, Esq., of Boston; Prof. F. E. Nipher, of the Missouri Weather Service; J. H. VanDoren, Esq., of Morristown, N. J.; to the Superintendent of the Meteorological Service at Toronto, and to Pres. J. W. Dawson, of Montreal, for assistance in collecting information.

1878, Jan. 1. An earthquake in Peru at 1.05 A. M., Lima time.

1879, April (day not stated). Earthquakes at Chilpancigo, on the west coast of Mexico, about 75 miles inland from Acapulco. —From Ann. Report of Dr. Fuchs in Miner. u. Petrog. Mittheilungen, Vienna.

May 17. The shock at Vera Cruz and vicinity, previously noted (xix, p. 227) as 16th or 17th, is given by Dr. Fuchs as 17th.

June 8. A shock at 6 A. M. at Cerro de Pasco, Peru.

Aug. 15. A shock at 6.10 A. M. at Cerro de Pasco, Peru.

Dec. 21. The period of disturbance which began on this date in San Salvador, C. A. (xix, pp. 299, 415) continued for several weeks, and during the last ten days of 1879 it was estimated that over six hundred shocks occurred, of which, however, only two were destructive, viz: those at 12.38 P. M., Dec. 27th, and 7.34 P. M., Dec. 31. The center of disturbance was Lake Ilopango and the damage was confined to its immediate vicinity. On Jan. 20th a new volcanic cone appeared in the lake, a description of which, with illustration from photograph, appeared in Nature (June 10, 1880). Mr. W. A. Goodyear, State Geologist of San Salvador, made careful observations of the phenomena, and his interesting report thereon has been published by the government of San Salvador in a pamphlet of thirty pages. In the expectation that his results will in due time be printed in English, further details are omitted here.

1880, Jan. 4. In connection with the eruption of volcanic dust in Dominica, W. I. (xix, p. 426); a letter from Mr. L. Bert (Comptes Rendus, Mar. 15, 1880) reports an earthquake shock at 11 A. M. at Marigot, a small village on the flank of the mountain chain in which the crater is situated.

Jan. 28, 29 and Feb. 10. Shocks and rumbling reported from Bald Mountain, N. C.—*J. M. B.*

Feb. 5. Advices of this date report recent earthquakes in various parts of Mexico, particularly the districts of Cordoba, Orizaba, Tehuacan and Vera Cruz.—Nature.

Feb. 8. The shock at Ottawa previously reported in small type (xix, p. 299) is confirmed by letter.

March 21. At 6.25 A. M. a heavy shock at Los Angeles, Cal., lasting about five seconds, vibration N.E. to S.W.—U. S. Weather Review.

March 25. At 2.30 A. M. a moderate shock at San Gorgonia, Cal., duration about three seconds, direction S.E. to N.W.—U. S. Weather Review.

April 3. At 10 P. M. a slight shock at Quebec and Ottawa.—J. W. D.

Same day, between 2 and 3 A. M., a shock at Fort Fairfield and Maysville, Me. —*J. M. B.*

April 14. A strong shock at San Francisco at 1.05 P. M.

May 5. A slight shock reported at San Francisco, from south to north, at 11 P. M., and at San Jose at 11.35 P. M. A heavy shock, presumably the same, was felt at Tlactula, State of Oaxaca, Mexico, on the same day, hour not stated; direction north to south.

May 12. At 7.45 A. M. an earthquake was felt in northeastern Massachusetts, affecting the seaboard towns from Amesbury and Newburyport to Salem, and inland to Lawrence and Acton. At

most places a rumbling sound was heard. The shock continued about five seconds.

June 24. At 12.47 A. M. a shock at San Francisco.—U. S. Weather Review.

June 29. The eruption of the volcano Fuego, in Gautemala, which began at 3 A. M. on this date, was preceded by violent earthquakes, whose effect, however, was confined to the surrounding country within a radius of twenty or thirty miles.

July 12. About 11 P. M. a slight shock at Concord, N. H.

July 13. About 8½ P. M. a shock occurred at Memphis, Tenn. and at Gayoso, Mo. Mr. H. Dow reports the time at Memphis as $8^h\ 24^m\ 52^s$, and Mr. H. Tresenriter at Gayoso at $8^h\ 40^m$, both local time. It was from N.W. to S.E. and lasted several seconds. The U. S. Weather Review notes two shocks at Memphis with an interval of fifty seconds.

July 16. At 10.25 P. M. (Washington mean time) a slight shock at Kingston, Jamaica, oscillation from north to south, lasting about three seconds.—U. S. Weather Review.

July 20. About 7 P. M. an earthquake occurred at Manchester, Milford, Contoocook and Antrim, N. H.; at the latter place two shocks.

July 21. At 10.50 P. M. at Valparaiso, Chili, a "long and rather strong" shock followed by several slight ones during the night.—N. Y. Times.

July 22. At 2 A. M. a shock at Ottawa, Ont., from west to east with rumbling noise.

Aug. 1. At 7 P. M. a heavy shock at Caracas, Venezuela.—*Nature.*

Aug. 10. About 12.15 P. M. a slight shock at Morristown, Dover, Mendham and vicinity in Northern New Jersey, accompanied by a noise as of a "distant explosion."

Aug. 14. A violent earthquake occurred in Chili from Valparaiso to Coquimbo and inland. The duration was nearly ninety seconds. No serious damage was done at Valparaiso, but churches and buildings were overthrown in Quillota and other towns near; and the town of Illapel suffered very severely.

Aug. 18. Two distinct earthquake shocks were reported from St. Dorothy district in Jamaica, during the severe cyclone which swept over the island on that night.—*U. S. Weather Review.*

Aug. 20. At 6.30 A. M. strong shocks were felt in the Vuelta Abajo region, in Candelaria and San Cristobal, in Cuba. The oscillation lasted seven seconds and was from east to west.—N. Y. Tribune.

Aug. 21. A shock at Barrington, N. H.—*U. S. Weather Review.*

Aug. 22. At 1.23 P. M. an earthquake was felt at Victoria, (British Columbia) and many other places in the southern part of Vancouver Island, and also at Port Townsend, Seattle and other points in the northwest of Washington Territory. At Victoria, two lighter ones were felt at 2.10 and 2.19 P. M.

Aug. 29. At 1.10 P. M. a slight shock at San Diego, Cal.—U. S. Weather Review.

Aug. 30. Several earthquake shocks about 2 or 3 A. M. were reported from Nonsuch Island and St. David's Island, in the Bermudas, during the severe cyclone which passed over the Islands on that day.—U. S. Weather Review.

Sept. 1. About ten or fifteen minutes before 5 A. M. a very slight shock was felt in Morristown and Dover, N. J., and vicinity. It lasted about ten seconds with a distant rumbling sound.

Sept. 6. A slight shock a little after midnight at Montreal, Huntington and Cornwall, on the St. Lawrence. The time given at Huntingdon is 0.30 A. M.; at Cornwall, 2 A. M.; and at the former a "rushing noise loud enough to awake every one" is mentioned.

Sept. 16. At 10.27 P. M. a shock lasting fifteen seconds, was felt in Utah, at various places from Salt Lake City to Provo. The movement was from S.W. to N.E., and in some places a low rumbling noise preceded it. At Provo a second shock was felt about 1 A. M.

Sept. 23. About 6 P. M. a shock at Charlotte, Vt.—*U. S. Weather Review.*

Sept. 26. At 5.40 P. M. a shock at Los Angeles, Cal., from west to east, lasting three seconds.—U. S. Weather Review.

Oct. — Middle of the month, the Ship "Ivy" experienced an earthquake shock when off the coast of Chili.—*San Francisco Bulletin.*

Oct. 26. At 1.30 P. M. a severe shock was felt at Sitka, Alaska, followed half an hour later by a second shock, and by seven or eight more in the succeeding forty-eight hours. The first and most violent shock continued twenty or thirty seconds, none of the others more than two or three seconds. This earthquake appears to have been felt along the coast of British America and to have been accompanied by a tidal wave, of which, however, no details have been received.

Nov. 4. At 7.37 P. M. a sharp shock at San Francisco and vicinity, vibrations east to west, duration five seconds; felt also slightly at San Jose.

Nov. 6. Under this date it is reported that "a shock of earthquake has been felt at Newcastle, Ont."—*Princeton Press.*

Nov. 12. At 8.45 P. M. at Los Angeles, Cal., a slight shock, duration three seconds.

At 10.30 P. M. the same night, a shock was felt at Santa Barbara, direction N.E. to S.W. lasting several seconds.

Nov. 21. At Los Angeles, Cal., and places south and east, three shocks were felt in the evening. The times are differently reported as 8.10 and 11 P. M. and 2.30 A. M., and from other sources as 7.45, 9.45 and 11 P. M. The first shock was the longest and most severe.

Nov. 24. At 11.45 P. M. a shock at Quebec.—J. W. D.

Nov. 28. At 8.30 A. M. a shock at St. Paul's Bay on the St. Lawrence.

Dec. 7. At 5.54 P. M. a slight shock from the S.W. at Olympia, W. T., lasting a few seconds; felt also at Bainbridge Island, where the direction was N. to S.

Dec. 10. A shock at 5 A. M. at Bainbridge Island, W. T., motion perpendicular.—U. S. Weather Review.

Dec. 12. About 8.40 P. M. a severe shock occurred in the vicinity of Puget Sound, W. T. It was felt from Victoria on the north to Portland on the south. At Olympia four shocks were reported, lasting ten or fifteen seconds. At Seattle the direction was S.E. to N.W.; at Bainbridge Island it was N. to S.

Dec. 14, 20. Slight shocks were felt at Bainbridge Island, W. T., at 7 P. M. of the 14th, and 11.16 P. M. of the 20th.—U. S. Weather Review.

Dec. 19. Between 2 and 3 A. M. a shock occurred at Los Angeles, Cal. At 3.40 P. M. another felt from Los Angeles to San Diego and vicinity, oscillations S.E. to N.W.

Dec. 21. At 11 P. M. a sharp shock at San Diego and Campo, Cal., from S.E. to N.W. At the latter place another similar shock, accompanied by a rumbling noise, occurred at 3.22 A. M. the next morning.

Dec. 26, 28. At Tecaluma, San Diego Co., Cal., a slight shock at 2.30 P. M. on the 26th and a severe one at 11 P. M. on the 28th. —U. S. Weather Review.

Dec. 29. At 11.25 P. M. a slight shock at Bainbridge Island, W. T.—U. S. Weather Review.

1881, Jan. 20. At 9.40 P. M. a decided earthquake, lasting about ten seconds, shook the vicinity of Bath, Me. It was felt at Brunswick, Bowdoinham and other places as far as Portland and Lewiston.

Princeton, N. J., Jan. 29, 1881.

Art. XXIV.—*On liquid Carbon dioxide in Smoky Quartz;* by George W. Hawes.

The presence of two immiscible fluids in the cavities of certain minerals has been so long known, and the number of such occurrences has been so multiplied by microscopic study of the rocks, that no especial interest is attached to new discoveries of this nature, save as they have a geological significance, or are associated with mineral peculiarities. I wish to show that such inclusions are common in smoky quartz, and to call attention to some occurrences which are very remarkable for the number and size of these inclusions. As my supply of excellent specimens is nearly limitless, a knowledge of these occurrences will be welcome, since most of the specimens of this nature that have been described contain only cavities that are minutely microscopic.

On examining the specimens of quartz in the cabinet of Professor Brush a number of crystals were found to contain cavities in which were included two fluids, one of which had the properties of carbon dioxide. All of these crystals had a smoky tint, and this I consider a circumstance of interest, as connected with the observation of Forster * that such crystals contain organic compounds. Forster found that from dark smoky quartz he could distil a brown fluid which had an empyreumatic odor, and which gave reactions for ammonia and carbonic acid; that a sooty deposit formed on the neck of his retort, and that the quartz was decolorized by this distillation. He therefore concluded that the coloring matter was a nitrogenous hydrocarbon decomposable by heat.

I have not considered it necessary to make any extended series of observations, since the few specimens examined indicate sufficiently that such inclusions are not rare in smoky quartz. The following localities furnish specimens with very large cavities.

Pike's Peak. Cavities ½ mm. in diameter and smaller. Some cavities contain only liquid carbon dioxide and its vapor. Some contain apparently only water. Some cavities contain cubic crystals in the water, and some are crystalline in form.

White Plains, North Carolina. Cavities fewer than in the preceding.

Monte Sella. Same as in the smoky quartz of Pike's Peak.

Fibia, St. Gothard. Cavities 2 mm. in longest diameter, large enough to be studied with a simple pocket lens.

* Pogg. Ann., cxliii, 173, 1871.

The smoky quartz from Branchville, Conn.,* is, however, the most wonderful in the number, size and diversity of the included cavities, and as I think that no specimens of this nature that are so remarkable have before been found, these deserve a description. In the great pegmatite vein from which Professors Brush and Dana have taken so many new minerals, smoky quartz occurs in considerable quantity, which is so full of cavities containing condensed gas, that a report like the explosion of a percussion cap takes place when a fragment is knocked off with a small hammer. When heated it decrepitates with such violence that bits fly whistling through the air to a distance of twenty feet. Any little chip broken off with a hammer will present under the microscope the appearance represented in fig. 13. Besides these cavities the quartz shows but few inclusions. These are in part pyrites, and in part iron oxide, and some appear to be black coaly particles. Some of the specimens are entirely free from all inclusions save the fluids in cavities. The quartz has a sp. gr. of 2·625, which is lower than that of pure quartz.† It has a very well marked rhombohedral cleavage, all three planes of the primary rhombohedron being frequently finely developed on pieces broken off at random.

Fig. 1 represents a cavity in which the outer zone is water, the middle liquid carbon dioxide, and the inner gaseous carbon dioxide. When such a cavity is gently warmed, the carbon dioxide in liquid form expands causing the inner zone to contract, and the liquid will expand sufficiently to occupy the whole space, causing the inner zone to disappear at a temperature lower than 31° C.

Fig. 2 represents a cavity of the same nature save that the inner zone is relatively larger. When warmed, the expansion of the liquid CO_2 is counterbalanced by the evaporation into the zone of the vapor within, and no especial change is noted till the critical point of carbon dioxide is reached, which is 31° C., when the inner and middle zone suddenly become one. The disappearance and reappearance of the inner bubble on cooling are, in the large cavities, attended with violent ebullition.

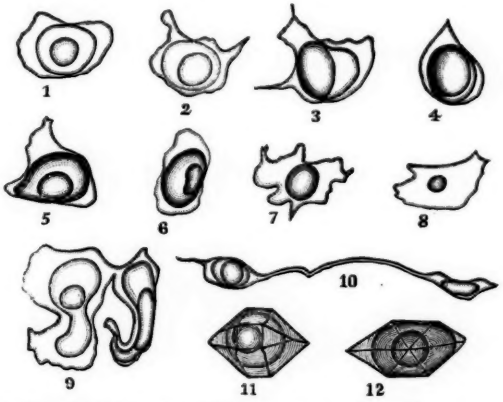
Figs. 3 and 4 represent cavities in which the bubble of vapor predominates more and more over the liquid carbon dioxide. In such cavities the bubbles will begin to expand as soon as heat is applied, and the whole fluid will evaporate into the inner zone, causing the bubble to disappear at a temperature below the critical point of the CO_2.

Figs. 5 and 6 represent cavities in which there is not sufficient liquid carbon dioxide to wet the walls between it and the

* The quartz that contains these inclusions is, however, not all smoky.

† Sp. gr. of pure quartz 2·65. Smoky quartz of Forster (l. c.) 2·65027.

water. It therefore gathers into a globule, and the liquid now takes its place as the inner zone. In such cavities again the inner zone grows smaller and disappears at a temperature below the critical point of the carbon dioxide, and the smaller the inner globule is, the quicker it will evaporate into the zone of vapor. In such cavities the line which divides the water from the middle zone is much blacker, and more deeply shaded, because the difference in the indices of refraction of water and vapor of carbon dioxide is much greater than between water and liquid carbon dioxide.



Cavities in Branchville quartz containing water and liquid and gaseous carbon dioxide in variable proportions, magnified fifty diameters.

Figs. 7 and 8 represent cavities in which if carbon dioxide exists, it is not in sufficient amount to be condensed to liquid form, and in which the volume of water is relatively much larger. These cavities, therefore, possess but two zones, the outer of water, the inner of vapor. The temperature at which such bubbles disappear is very much higher and dependent upon their size.

No relationship between the position of the cavities and the relative size or method of arrangement of the three zones could be detected. The cavities figured form an interesting series, dependent upon the relative volumes of the two fluids, and any or all the members of the series are liable to be simultaneously present in the field of the microscope. The temperatures at which the inner bubbles disappeared and reappeared in cavities simultaneously visible were noted as follows:

	Disappeared.	Reappeared.
(1)	24°	24°
(2)	25°	25°
(3)	27°	27°
(4)	30°	30°
(5)	31°	31°
(6)	32°	32°
(7)	110°	25°
(8)	114°	After more than two weeks.

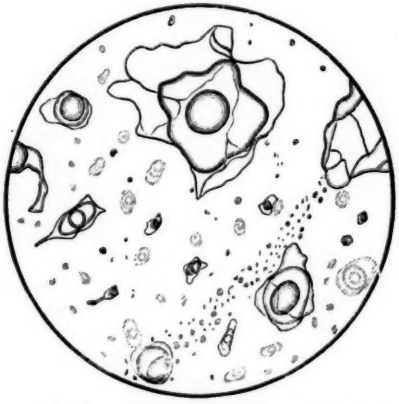
The cavities numbered from 1 to 6 contained three zones and the bubbles disappeared and reappeared as described; but the cavities numbered 7 and 8 were like figure 8 and possessed but two zones. The bubble which disappeared at 110° returned suddenly as a cloud of minute bubbles like the bursting of a sky rocket when the specimen had cooled to 25°. The bubble which disappeared at 114° could not be induced to return by long immersion in a freezing mixture. At the end of two weeks it had not returned, and it was only by chance that after this article was complete, and I had written that it had finally disappeared, that I discovered that it had returned in full size. I have thought these phenomena might be explained as follows: The inner bubble is not, as in the other cases, a space filled with the vapor of an expansible liquid with which it is in contact, but it is a space filled with the vapor of carbon dioxide in contact with water. When heated the water can expand and fill the space, but it must act not merely by virtue of its expansibility to do this; it must also condense or dissolve this gas, and under the generated pressure it could dissolve it. The conditions for the reappearance of the bubble on cooling would be different from those which exist when a space is formed by the contraction of a homogeneous liquid. The dissolved gas might separate from the contracting liquid, as a multitude of bubbles adherent to the walls of the cavity, and too minute to be detected by the microscope. The bubble which disappeared at 110° and returned at 25° came back as a large number of small bubbles which only reunited to one after being jostled about for an hour or more, and the bubbles may have returned in the other cavity in still greater number, forming an undetected film upon some portions of the walls.

Figures 9 and 10 represent some of the strangely deformed cavities, which exist in limitless diversity in this stone. In the cavity shown in figure 9 there are two connected chambers, each of which contains the three zones. But owing to the difference in relative volume of liquid and gaseous carbon dioxide, on warming, the bubble in the left chamber contracted and the one in the right expanded at the same time. After both had disappeared, on cooling both reappeared, but their relative volumes had changed, the bubble to the left becoming twice as

large as formerly, and the one to the right becoming correspondingly small. A part of the water had been transferred therefore to the right hand chamber

Many cavities, especially those in well-developed crystals, are bounded by planes parallel to the outer planes of the crystal. Such are represented in figs. 11 and 12. These usually appear dark in color, by transmitted light, because so much light is reflected away by the crystalline planes. Their inclusions are frequently better seen by reflected light. These cavities attain sometimes to a diameter of two millimeters.

13.



A fragment of Branchville quartz as it appears when magnified twenty-five diameters.

Fig. 13 represents the general appearance of the rock, as seen with a low magnifying power, a cavity 1^{mm} in diameter being in the field. Such a bubble as this cavity contains will contract when heat is applied, but before it disappears the critical temperature will be reached when the bubble will expand and occupy all the space of the two inner zones. The most violent ebullition will occur when the section cools to the critical temperature. A multitude of small globules will appear, and will finally unite, and the reverse processes of contraction and expansion will follow.

The character of the included gas has been determined by Professor Wright, who gives the results of his investigation in another article.

Having observed bubbles both in liquid carbonic acid and in water that were in rapid erratic motion, the old question of the cause of these motions was suggested. The lack of a conclusive explanation of these movements has never been felt

since the phenomenon is physical, and has no special bearing upon geological science, further than that the motion furnishes a ready means for the demonstration of the presence of a mobile fluid, in distinction from glass. A sufficient number of explanations have been offered, yet so late a book as the Minéralogie Micrographique* of Fouqué & Levy, gives a choice of explanations, and an admission of doubt.

The following experiments I think may form the basis for an opinion. If a cavity containing a bubble of vapor floating in a fluid is warmed upon one side, the bubble will move toward the source of heat. If the enclosing fluid be carbon dioxide, this movement is produced by a very slight change in temperature. With a globule of hot glass on the end of a rod such a bubble can be brought into a slow vibration, by moving the source of heat from side to side. The motion will not alter in direction when the bubble has become very small, by the expansion of the containing fluid, but the velocity of the motion is much increased. If the containing liquid be water, bubbles of large size will not be affected so quickly, but when the bubbles have become small, they move with great rapidity.

In water cavities, the bubbles of which have been absorbed by the expanding liquid, the bubbles appear upon the colder side, and shoot rapidly toward the warmer side of the cavity.

Currents of heat can then produce very rapid motion in bubbles of small size, and these motions are definite in direction, when the direction of the current is definite. They can all be explained as being caused by the evaporation of the liquid into the space, the evaporation taking place upon the warmer side, and condensation upon the colder side. When one thinks of the exquisite balance which exists between the temperature and the number of molecules which exist in a closed space within a fluid acted upon simply by the pressure of its own vapor, one can imagine the difficulty of restraining action, for physicists know what great precaution is necessary to obtain anything like a uniform temperature; a condition almost impossible to secure on the stage of the microscope. The following experiments were tried with the object of observing the effect of an approximately equalized temperature.

A section of gold quartz, containing most active bubbles included in liquid carbon dioxide, was placed in a tank of water upon the stage of the microscope. The instrument was placed in the middle of the room and the window was opened to equalize the temperature within and without. Four cavities were especially noticed in the field of view. The bubbles all possessed different rapidity of motion. After standing over

* Minéralogie Micrographique Roches Eruptives Francaises, Paris. 1879.

night the vibration of the two bubbles having the slower rates of motion had ceased.

A specimen of granite containing very movable bubbles, included in water cavities, was likewise treated. In the morning the motion had ceased in the cavity especially noticed, but it began again after I had stood by the microscope a moment.

I think that these experiments are sufficient to show that currents of heat, by producing alternations of evaporation and condensation, can cause motions like those seen in the bubbles floating in these fluids, and that the changes of temperature sufficient to move these bubbles are very small in amount, and very difficult to avoid. This explanation is not new, and would, I think, suggest itself to most physicists, but it has not been shown to be sufficiently probable to receive acceptation.

A result of my examination of the cavities in quartz from Branchville has been most plainly to identify these cavities as being of the same nature as those that have been studied by several eminent microscopists in topaz and other crystals, and in granites, gneisses, basalts and other rocks. Quantities of condensed gas sufficient for exhaustive analysis have not however been heretofore found, and therefore the associated communication of Professor Wright will have a general interest to geologists, since it bears upon a class of long studied phenomena.

ART. XXV.—*On the Gaseous Substances contained in the Smoky Quartz of Branchville, Conn.;* by ARTHUR W. WRIGHT, Yale College.

THE existence in quartz of numerous cavities containing a liquid substance is a matter of familiar occurrence, and great interest has attached to the investigation of the character of these inclusions. Although the presence of carbon dioxide and water had been well established, the difficulty of separating the contents of the cavities in sufficient quantity has hitherto prevented a direct examination of them. The quartz from Branchville is remarkable for the great size and number of the cavities, the peculiar characteristics of which are described by Mr. Hawes in the preceding article. The fortunate circumstance, noticed by him, that when exposed to a moderately high temperature it decrepitates and is speedily resolved into small fragments, made it possible to obtain with great ease and convenience enough of the enclosed substances for an extended examination. The material employed was derived from the collection of minerals from Branchville, of Professors Brush and Dana.

The temperature required for the disintegration of the quartz is much below that of red-heat, and the bursting of the solid material is evidently due to the increased tension of the gas, as it does not occur in those fragments which contain no cavities. The first trials were made with glass vessels, but the sharp fragments of the mineral were shot off with such violence as to destroy them immediately. Recourse was therefore had to a porcelain tube about one centimeter in diameter, glazed inside. This was carefully cleaned with pure distilled water, one end stopped with a plug cemented in, and the other provided with a perforated brass cap, into which could be screwed a piece through which passed a slender glass tube, the joint being rendered tight by a thin washer of india-rubber or paper. The closed end of the tube was filled for some 12 centimeters with pieces of clean glass rod, and upon these rested a loose plug of calcined asbestus. The quartz, broken into fragments of such a size as to permit their entrance, was dropped into the tube, filling it to within 10 or 12 centimeters of the mouth. When heated in a Bunsen flame, the whole of the material could be brought to the requisite heat without causing any perceptible elevation of the temperature of the cement joints. This receptacle, when charged, was connected by means of the glass tube with a Sprengel pump, all joinings of the glass tube being made by fusing, and the whole was easily rendered absolutely free from leakage.

The pump having been kept in action until no gas appeared to pass down, heat was cautiously applied to the tube, and gradually increased until a little gas was liberated from the quartz. When this had been thoroughly pumped out, removing thus the last portion of air, the heat was again applied and continued until the cessation of the decrepitation showed that no more gas could be obtained. The mercury was then set running in the pump carrying the gas into the measuring tube used for the analysis. A preliminary examination showed the greater portion of the gas to be carbon dioxide, the remainder apparently consisting chiefly or wholly of nitrogen. A considerable amount of water was also found to be present. In the succeeding operations this was collected for examination by causing the gas as it issued to pass through a U-tube of small caliber which was placed in a freezing mixture. As the temperature of the refrigerating mass was such as to reduce the tension of the vapor to less than one millimeter nearly the whole of the water was thus retained.

For the more careful analyses two portions of the rock were selected representing the greatest differences in the material. The first, No. 1, was of a light gray color, somewhat milky in appearance, and contained many cavities easily visible without

the aid of a lens. The weight of the material employed was 21·70 grams, which, divided by the specific gravity 2·63, gives for the volume 8·25 cubic centimeters. The second portion was of the darker variety, having a smoky brown color, appearing nearly black in large masses. The gas cavities in this were not so conspicuous, and apparently were less numerous. The amount of the material placed in the tube for examination was 19·49 grams, and the volume 7·41 cubic centimeters. This portion is designated as No. 2 in the following paragraphs. The total quantity of gas collected from No. 1 was 13·61 cubic centimeters, or 1·65 times the volume of the quartz. From No. 2, 7·20 cubic centimeters of gas were obtained, or 0·97 times the volume of the material employed. From the first portion examined in the preliminary work 1·33 volumes were obtained.

The eudiometer having been transferred to the mercury cistern, an absorption pellet moistened with solution of potassic hydrate was introduced into it, causing a rapid diminution of the volume of the gas. When this operation was complete the residual gas had been reduced to a small bubble in the top of the tube, which could not be measured directly with sufficient accuracy. To find its volume a little of the potash solution or of distilled water was admitted giving a meniscus concave toward the top of the tube. The position of this was carefully noted, and the tube emptied. Mercury was now introduced until the surface of the meniscus occupied exactly the former position of the surface of the water, and the metal was then weighed. The mean of five separate measurements being taken the volume of the gas was thus readily calculated. The results of the determinations with the two different portions of the material gave

	I.		II.	Mean.
CO_2	98·34	CO_2	98·32	98·33
N	1·66	N	1·68	1·67
	100·00		100·00	100·00

Cuprous chloride produced no perceptible absorption, showing the absence of carbonic oxide. Potassium pyrogallate introduced into the tube with caustic potash solution produced a slight discoloration of the latter, but no change in the volume of the gas was visible, indicating that oxygen if present was not in recognizable quantity.

To ascertain the presence of hydrogen or other combustible gases a number of tests were made. A spark passed through the gas directly produced no effect, nor upon the addition of oxygen or air alone could any combustion be produced. When the proper quantity of pure electrolytic gas was added

the explosion produced no apparent change in the volume. This, if, from the small amount of gaseous substances operated with, it might not safely be concluded that hydrogen or hydrocarbons were entirely absent, shows that the quantity was exceedingly small. The residual substance then was nitrogen. In these operations, as already mentioned, the gas had passed through a tube placed in a freezing mixture. A later experiment, to be described in a succeeding paragraph, gave a somewhat different result.

The rock when broken or crushed with a hammer exhales a fugitive but unmistakable odor of hydrogen sulphide, but the proportion of the gas was too small to be directly detected with the ordinary lead-paper even when directly applied as a cover to a diamond mortar in which a considerable quantity of the material had been powdered. But when a slip of the paper was introduced into a tube filled with the extracted gases, a slight but distinct coloration was produced. The same was true in a somewhat more marked degree with a paper moistened with mercurous nitrate, indicating sulphurous oxide. To test this more fully separate slips of filtering paper were wet with plumbic acetate, sodium nitro-prusside, and mercurous nitrate. When dry they were introduced into a small tube through which gas freshly liberated was made to pass. Snow applied for a few moments to the tube ensured the presence of sufficient water to moisten the paper slightly. The first underwent a slight discoloration, which after a time disappeared, the second assumed a pinkish tint, while the third was distinctly blackened, thus proving the presence of a trace of both the gases in question, a conclusion moreover which was verified by other and independent trials.

As both hydrogen sulphide and sulphurous oxide are absorbed by potassic hydrate it was important to ascertain whether these gases were in sufficient quantity to affect the conclusion given above as to the amount of carbon dioxide. A portion of the gas collected in a clean tube was therefore submitted to a special examination. A pellet of ferric oxide formed upon the end of a platinum wire produced no effect at all, though kept in the gas for several hours. A similar pellet of manganese dioxide moistened with syrupy phosphoric acid likewise caused no perceptible effect, thus proving that these gases were not present in any measurable quantity.

An approximate estimation of the amount of water was made as follows: The U-tubes in which the water had been condensed were sealed after the gas had been thoroughly pumped out. The temperature of the freezing mixture was from $-19°$ to $-20°$ C., so that the tension of the residual vapor was less than one millimeter, which was confirmed by the reading of

the guage of the pump at the end of the operation. The connecting tubes were fused off and the portion containing the water withdrawn. The amount of liquid thus obtained was considerable. The tubes were carefully weighed, then opened, and after an examination of the liquid, thoroughly dried, and weighed again. The weight of the water in No. 1 was thus found to be 13·4 milligrams, in No. 2, 12 milligrams, corresponding respectively to 13·4 and 12 cubic millimeters at 4°. The volume of the quartz in the first instance being 8·25, and in the second, 7·41 cubic centimeters, we have for the amounts contained in one cubic centimeter of the mineral, 1·63 and 1·62 cubic millimeters respectively, no correction being made for temperature, as the results are only approximate. This would indicate a comparative uniformity in the distribution of the water, while the amount of the gas varies. But such a conclusion is at best doubtful, inasmuch as the darker quartz is not as thoroughly broken up by the heat as the lighter variety, and the refrigeration of the tube No. 1 was not made complete at first, so that some water doubtless escaped with the gas uncondensed.

A small portion of the water removed with a minute pipette was dropped upon red litmus paper, where it produced a strong but fugitive alkaline reaction, implying the presence of free ammonia. This was confirmed by adding Nessler's test solution to the remainder of the liquid in the end of the tube, in which it caused the characteristic yellow coloration, and, in one instance, a slight precipitate. Before the tubes were opened it had been noticed that the water, though to all appearance perfectly transparent and colorless, left a white deposit upon the glass where a drop of it had evaporated. When this was heated by the application of a small gas flame, it did not fuse, but appeared to shrink or to diminish in amount very slightly, while the glass around it and over it lost its transparency as if corroded. A similar but very slight action upon the glass where the moist gas had come in contact with it had previously been observed. This suggested the presence of fluorine. The glass of the tube in which the effect was most marked contained some lead, but the other showed it also to some extent. A special experiment with a tube free from lead, which had been most carefully cleaned, gave the same result, though in a somewhat less marked degree. Its appearance would be accounted for by the supposition that the water of the cavities contained some hydro-fluo-silicic acid in solution, resulting from the decomposition of silicon fluoride, or, as ammonia was also present, from an ammonium compound of the acid.

As was mentioned in a preceding paragraph, no evidence of the presence of a hydro-carbon compound was discovered in

the examination of the gas which had passed slowly through the cooled tubes. The tubes themselves, however, contained what appeared like minute drops of some oily substance, so small as to be scarcely visible without a lens, and quite insufficient for examination. In order to investigate this point more satisfactorily, as also to obtain a greater quantity of the residual gas left by absorption of the carbon dioxide, an experiment was made as follows: A bolt-head of porcelain, glazed interiorly, and having a capacity of about 300 cubic centimeters, was employed for the reception of the quartz, of which 196 grams, making 75·4 cubic centimeters, were used. It was arranged that after the air had been pumped out, the gas from the quartz should pass through a strong solution of potassic hydrate contained in a large U-tube, all connections being made with fused glass joints as before. The greater portion of the carbon dioxide was thus absorbed. Unfortunately just at the close of the operation a slight crack in the porcelain vessel admitted some air, but the tube leading to the pump was sealed immediately, so that the amount mixed with the gas was not too great to permit a quantitative examination of the gas to be made. A portion of the latter being transferred to the eudiometer, and just sufficient electrolytic gas being admitted to ensure combustion, the volume of the gas after explosion was found to be considerably increased, with the production of carbon dioxide. Repeated tests gave uniformly the same result, but the expansion was greater at first than after the gas had been kept for two days in the pump. This must be regarded as evidence of the presence of the vapor of some condensable hydrocarbon having a large number of carbon atoms in the molecule.

The quartz on heating entirely loses its color, the coarse powder which is left being almost snow-white. Now, in the experiment just described, a dark brownish deposit was formed in the tube leading from the bolt-head, and the potash solution after the passage of the gas had become brown, the color being almost exactly the same as that of the quartz before the heating. After standing a day or two a small amount of a dark brown, nearly black, substance separated out as a precipitate and the liquid lost its color. The potash solution was now decanted and the dark deposit examined. Treated with alcohol it dissolved but partially, communicating its color to the liquid, and taking on a tarry consistency. On evaporating the alcohol, the substance was volatilized by more intense heat, with a strong bituminous odor, very much like that given off by cannel coal when burning. The brown deposit in the tube also gave off the same odor when strongly heated. These results imply that the smoky color of the quartz is due to the

presence of a hydrocarbon of the nature of bitumen, which is driven off by heat, and the partial decomposition of which, at the high temperature reached, accounts for the heavy hydrocarbon found in the residual gas, or condensed upon the walls of the cooled tubes. These facts, moreover, are entirely in harmony with and confirm the conclusion of Forster* from an examination of the remarkable smoky quartz from the canton of Uri, that the color of the latter is due to the presence of some volatizable hydrocarbon, though they do not directly connect the ammonia with the latter, as his observations appear to do.

After the operation just described had been concluded, some pure distilled water was introduced into the bolt-head, and after standing for some time was then withdrawn. Tested with argentic nitrate it gave a considerable precipitate of argentic chloride, while when examined spectroscopically it afforded satisfactory evidence of the presence of sodium, but of no other metal. The water previously examined was found to be free from both chlorine and sodium. The bolt-head had been scrupulously cleansed before use, and great care was taken in this, as in all the experiments, to prevent contact of the quartz with things that might communicate to it any impurity. This result would indicate that the cubical crystals observed by Mr. Hawes in some of the cavities were chloride of sodium. Search was also made for chlorine or chlorine compounds in the gas. A quantity of this freshly liberated was passed through distilled water. This, on the addition of argentic nitrate, was very slightly clouded, making the existence of a trace of some chlorine compound probable. Not unlikely a minute proportion of ammonium chloride is among the contents of the cavities.

The quantitative relation of the water to the gases obtained from the quartz may be made more evident if calculated for a temperature of 100° C. at which the former would be entirely converted into vapor. Taking the amount of water per cubic centimeter at 1·62 cubic millimeters as found above, this multiplied by 1694·3 gives 2·74 cubic centimeters for the volume of the water vapor at 100°. If we take the gaseous volume for one cubic centimeter of the quartz at 0·97 cubic centimeter, the result derived from No. 2 above, where the water determination was most satisfactory, the temperature of the room at the time of measurement being about 20° C., we have for the volume at 100°, neglecting the correction for the barometric pressure which was not greatly different from 760 mm., 1·23 cubic centimeters. Reduced to parts in 100 these volumes give

* Pogg. Ann., cxliii, 173, 1871.

CO_2	30·48
N	0·50
H_2O	69·02
	100·00

For the reasons mentioned above this must be regarded, so far as the water is concerned, as merely an approximate result.

For the gases alone, leaving out of view the bituminous matter, which is not known to be specially connected with the cavities in the material, and probably is not, we have the following summary:

CO_2	98·33
N	1·67
H_2S	*trace*
SO_2	"
H_2N	"
F	"
Cl?	"
	100·00

The ammonia, if derived from the gas cavities, undoubtedly existed there in combination with the carbon dioxide, as ammonium carbonate. From the considerations mentioned above the fluorine and chlorine detected by the tests applied also represent compounds of these elements with some of the other substances present. The results of the investigation show that the contents of the cavities are chiefly water and carbon dioxide, with a small portion of nitrogen, thus essentially confirming the conclusions derived from microscopical examination.

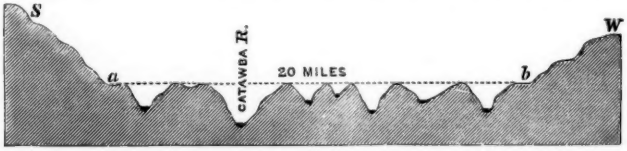
Yale College, Feb. 11, 1881.

ART. XXVI.—*Origin of some new points in the Topography of North Carolina;* by W. C. KERR, State-geologist of North Carolina.

ALTHOUGH all the ordinary indications of glaciation are wanting in North Carolina, even in the higher plateaus of the western mountainous region, as ascertained and announced more than ten years ago, yet, singularly enough, long after I had ceased to look for evidences of glacial action, I have arrived, by two entirely independent lines of investigation, at the conclusion, unexpected and unsought, that glaciation has occurred here on a large scale, and at two different periods. Moreover, the evidences of the existence and action of glaciers is totally different from the commonly recognized marks and results of glacial action.

I have elsewhere described briefly one of these classes of phenomena, and shall discuss them more at length very soon; of the other I give here some outlines, sufficient to indicate the character of the evidence and to direct the attention of observers to similar phenomena elsewhere.

The accompanying diagram represents the facts better than any description can do. It is an ideal cross section of the hydrographic basin of the Catawba River, which takes its rise in numerous tributaries along the flanks of the Blue Ridge, and after gathering up a multitude of these, traverses the Piedmont, or cismontane plateau (of an elevation of 1,000 to 1,500 feet),



Ideal section of the Catawba River basin, between S., South Mountains, and W., Warrior Mountains.

in a wide basin or trough, flanked by two ranges of mountains, which rise on either hand to an additional elevation of a thousand feet and upward. The direction of the axis of this trough and of the bordering ranges is 60° to 70° east of north, which is about coincident with the strike of the rocks.

These rocks are Archæan — hornblendic and feldspathic gneisses, and micaceous and hydro-micaceous schists, of varying hardness and durability. The river and its several affluents have dug their channels along the softer and more yielding strata, and have left the tougher and more resisting masses in ridges and low spurs, knobs and domes, which rise above the beds of the intervening streams, from a hundred to three, four and five hundred feet. But the singular and significant fact, as indicated in the diagram, is, that to one standing at *a*, for example, a bench on the flanks of the mountains to the south, or at *b*, of the mountains to the north of the basin, and looking across, all the intervening ridges, knobs and hills *rise to the same level eye-line*, which might be represented by a water-line, so level does it seem from side to side; and one imagines that he is scanning a broad level valley, flat as a prairie. It is only at this particular elevation, about 1,400 feet in the meridian of Morganton,—less as you go east, more as you approach the Blue Ridge, with a very gradual ascent,—that the observation is practicable. A few feet lower you see nothing but the next ridges and hills on either hand; a little higher on the flanks of the mountains, above the line *a b*, nothing strikes the eye but a chopping sea of low mountain spurs

and knobs and hills and peaks, stretching away in confused and disorderly succession into the foot hills of the opposite bounding range. I had crossed this valley many times and in many directions, and ascended these mountain chains and measured most of their summits, mapping out in detail their intricate topography for half a dozen years, before this remarkable fact caught my eye. And it was not until the observation had been repeated from different points in the Catawba basin, and the same phenomenon had been observed on a grander scale in the great transmontane valley, the hydrographic basin of the French Broad,—a plateau of nearly a thousand feet greater elevation and of correspondingly greater breadth,—that the full significance of it began to appear.

In the summer of 1877, I stood on one of the foot-hills of the Black Mountains, near the head waters of Ivy River, within the horizon which cuts the level tops of the ridges and knobs and mountain spurs of this rugged and cañon-gashed valley-plateau. During the same summer, ascending the slope of the South Mountains, from a jutting spur, I happened to catch the summit level of the Catawba plateau, at a point which gave the whole sweep of it in length as well as breadth; it stretching westward thirty miles, to the foot of the Blue Ridge, and twice that distance eastward, until the last ridges of the Piedmont became merged into the western margin of the Midland plateau. One readily imagines the valley to have been an arm of the sea during some ancient period of subsidence, and these numerous hill-tops rising as islands to the surface, like the thousands of atolls in the Pacific; and the grand generalization of Darwin came into mind. But no subsidence or action of water seemed adequate to meet the conditions of this problem; nothing, it appeared to me, could explain it except the presence of a great glacier, which in some ancient time had moved down the valley and left the surface nearly level, and then, the subsequent action of meteoric agencies through long ages, channeling out of the plateau thus made the present rugged topography, leaving only this faint trace of its former extent. Nor does any other solution yet occur. The reasonableness of this hypothesis will appear, if it is considered what would be the consequence of the movement of such a glacier over the present surface of the valley for a few thousand years.

As to the time of this supposed glaciation, we note that the opinion seems to be gaining among geologists that there have been several, perhaps many, glacial periods, whether Croll's theory be accepted or not. And there is some ground for the view which introduces the Triassic period with an epoch of extensive glaciation, and the phenomena in question may be

connected with the rapid accumulation of the basal deposits of that formation. Or it may hereafter be ascertained, by a more minute study of the Appalachian regions of the middle latitudes, that glaciers established themselves at the higher levels in the earlier times of the Glacial period, but did not recur during the second accession of cold, while the more extensive and long-continued Diluvial denudations and erosions may have sufficed to remove the debris, striæ and other indications of earlier glacial action.

Raleigh, N. C., Dec. 24, 1880.

ART. XXVII.—*Occurrence of Realgar and Orpiment in Utah Territory;* by WILLIAM P. BLAKE.

THE minerals realgar and orpiment occur together in a thin bed or layer in the horizontal sedimentary formations underlying the lava of Coyote Mining District, Iron County, Utah. The formation, with its lava cap, forms the divide between the head-waters of the Sevier river and the Colorado drainage, and is known as the "Rim of the Basin." The horizontal beds of stibnite, recently described by Professor J. S. Newberry, occur in the same formation, but chiefly in a sandstone of a different horizon.

The arsenical sulphides are found in a compact, sandy clay, in a horizontal seam or layer about two inches thick, not distinctly separated from the clay, but lying in its midst, in lenticular and nodular masses. The bulk of the layer consists of realgar in divergent, bladed crystals, closely and confusedly aggregated, sometimes forming groups of brilliant crystalline facets in small cavities towards the center of the mass.

The orpiment, which is closely associated with the realgar, is in small and delicately fibrous crystalline rosettes, and small spherical aggregations, made up of fine radial crystals, and also, in bright yellow amorphous crusts in and around the mass of the realgar.

Above and below the layer, and in close contiguity, there are thin parallel seams of fibrous gypsum. The strata above, for thirty feet or more, are arenaceous clays charged with soluble salts which exude and effloresce upon the surface of the bank and form hard crusts. The whole appearance and the association of the arsenical sulphides indicate that these sulphides have been formed by aqueous infiltration since the deposition of the beds. This, I have no doubt, is the fact; and, also, that the antimony sulphide had a similar origin.

MILL ROCK, New Haven, Jan. 15, 1881.

ART. XXVIII.—*On the Solubility of Chloride of Silver in Water;* by JOSIAH P. COOKE. (Contributions from the Chemical Laboratory of Harvard College.)

THIS subject has already been studied by Stas, whose observations are summed up by Dr. John Percy* in his recent volume on the Metallurgy of Silver in the following words:

"The solubility of the chloride is greatest when in the flaky state, as precipitated in the cold from a sufficiently dilute solution of silver; the solubility diminishes as the flakes shrink when left to themselves, or as they are rendered pulverulent by long agitation with water. Flaky or pulverulent chloride of silver, dissolved in water pure or acidified by nitric acid, is precipitated by the addition of a salt of silver or of hydrochloric acid or of an alkaline chloride." . . . "The solution of the chloride is wholly effected by pure or acidified water, as the case may be, and is not caused by the soluble salt formed simultaneously with the chloride of silver. The presence of nitric acid in the water does not affect the solubility of flaky chloride of silver; but it increases the solubility of the pulverulent chloride in proportion to the quantity of acid present. The precipitation of the dissolved chloride is the exclusive result of its insolubility in the solution formed by adding an excess either of the silver salt or of the alkaline chloride."

So also in Liebig and Kopp, Jahresbericht, 1871, 339: "According to Stas, the granular scaly and crystalline chloride is wholly insoluble in cold water: in boiling water the solubility is comparatively great, but decreases rapidly with the temperature."

In our investigation on the atomic weight of antimony we have had occasion to confirm and extend these observations of Stas, and our results may be of interest as showing that in the very familiar method of determining chlorine by precipitation with nitrate of silver, which is generally supposed to be extremely accurate, a sensible error may arise from the solubility of the chloride of silver in the hot distilled water used in washing the precipitate. It would be well for every analyst to make the following very striking experiment, which will enable him to appreciate the extent of the action in question.

Take from five to ten cubic centimeters of pure hydrochloric acid and precipitate the chlorine in the usual way with nitrate of silver, avoiding a large excess. After pouring off the supernatant liquid and washing the precipitate once or twice with cold distilled water, pour upon the white flaky chloride of silver a comparatively large volume of boiling water. As soon

* Metallurgy of Silver and Gold. Part I, page 60.

as the precipitate settles, pour off the clear hot water, dividing the solution between two precipitating jars. To one of these add a few drops of a solution of nitrate of silver, and to the other a few drops of hydrochloric acid. In both cases a precipitate of chloride of silver will fall, and most chemists, certainly, will be surprised at the effect; for it is not a mere turbidness that results, but a well-defined precipitate, whose amount is easily estimated. Successive portions of boiling water poured upon the precipitate give the same reaction. In one experiment the reaction was still perceptible in the fourteenth wash-water. But under the action of the boiling water, the precipitate becomes crystalline or granular and the action lessens, until at last the water does not dissolve sufficient chloride of silver to cause even a cloudiness on the addition of nitrate of silver, as just described. Mr. G. M. Hyams, a student in this laboratory, washed two different portions of chloride of silver with boiling water until the action ceased, and then weighed and examined the residue. In the first experiment, 1·4561 grams of chloride of silver were washed with 66 liters of water. The chloride of silver was then collected and found to weigh 1·2320 grams. Hence, 0·2241 grams, corresponding to 15·39 per cent had passed into solution. In the second experiment, 60 liters of water were used, and 16·03 per cent of the chloride of silver originally precipitated were dissolved. These numbers, however, are only approximately accurate, for as the precipitate becomes granular, it settles with less readiness, and there was necessarily some loss in filtering off so large a volume of liquid.

In the experiments above described the boiling water produced only a very slight decomposition of the chloride of silver. The precipitate, granulated by the washing, readily dissolved in aqua ammonia, leaving less than a milligram of a black powder, which was proved to be metallic silver.

The solvent power of water on freshly precipitated chloride of silver did not appear to be influenced by the presence of free nitric acid even in large quantities. We tried the effect both of dropping the nitric acid on the precipitate before pouring on hot water, and also of previously adding nitric acid to the boiling wash-water. We used amounts of nitric acid ($\delta = 1{\cdot}355$) varying from five to two hundred cubic centimeters to the liter of water, but without finding any marked difference in the result.

The presence of a small amount of nitrate of silver in the water entirely prevented its solvent action, so far as we could discover. In order to determine the limit of the action, we added different quantities of nitrate of silver to the boiling water before pouring it on to the precipitated chloride of silver. With one centigram of nitrate of silver to the liter of

water, there was a marked turbidness on subsequently adding an excess of the same reagent to the filtrate. With two, three, or even four centigrams to the liter, an opalescence could still be distinguished, although constantly diminishing with the increasing amount of the salt. With five centigrams, there was no opalescence, and we concluded that one decigram of nitrate of silver to the liter of boiling wash-water would certainly prevent all action.

A few drops of hydrochloric acid added to the wash-water greatly diminishes its solvent action on flaky chloride of silver, but does not wholly prevent it, as is evident from the fact shown in the table below, that hydrochloric acid does not precipitate chloride of silver from its solution in water nearly as effectually as nitrate of silver; and, as is well known, hydrochloric acid if in any considerable excess exerts a strong solvent action on the precipitated chloride.

As shown by Stas, the precipitation of chloride of silver from its solution in hot water by the reagents we have named, depends solely on the change which the reagents produce in the solvent. That the action is an example of simple solution is shown by the fact that a considerable portion of the chloride of silver dissolved in boiling water is deposited when the solvent cools. This phenomenon is a striking one and can easily be observed by pouring into a glass crystallizing pan some of the clear solution obtained in the experiment described above. As the water cools it becomes cloudy and deposits a granular powder which adheres to the bottom of the glass. The grains are usually very small, but if the solution cools slowly the crystalline form can readily be distinguished under the high powers of a good microscope, and the little cubes present all the characteristic of the native crystals of chloride of silver. It is evident, therefore, that the granular condition of chloride of silver is a crystalline condition, and this experiment may elucidate the manner in which the native crystals are produced.

We have thus far only spoken of the solubility of chloride of silver in boiling water. As is evident from the crystallization just described, the solubility rapidly diminishes as the temperature falls; but even at the ordinary temperature the solubility is distinctly marked. Luke-warm water poured on and off, freshly precipitated chloride silver becomes decidedly opalescent on the addition of nitrate of silver, and even if cold water is used the opalescence is perceptible.

In order to obtain an approximate measure of the effects we have described, Mr. Hyams precipitated about fifteen grams of chloride of silver, and, after thoroughly washing it, boiled the precipitate with a large volume of water in a glass flask.

At the end of an hour he decanted through a filter about one liter of the boiling water, and, having divided the filtrate into two portions, he added to one portion nitrate of silver, and to the other, hydrochloric acid. The precipitated chloride of silver was in each case collected and weighed. At the end of two hours' boiling, two other portions were filtered off and treated in a similar way. These determinations were then repeated with a fresh quantity of chloride of silver, and afterwards taking with a third quantity of chloride of silver, the boiling water was simply poured on twice in succession and the similar portions thus obtained treated as before. The results in every case were nearly the same as shown in the following table. In this table

1 and 3 are results after one hour's boiling of 1st quantity.
3 and 4 " " " two hours' " " " " "
5 and 6 " " " one hour's " " 2d quantity, etc.
7 and 8 " " " two hours' " " " " "
9 and 10 after simply pouring on boiling water.
10 and 12 " " " " " "

No.	Wght. of Water.	Wght. of AgCl.	Wght. of AgCl per liter.	Precipitant.
1	523·6 gram.	0·0011	0·0021	Nitrate of silver.
2	469·5	0·0004	0·0009	Hydrochloric acid.
3	115·0	0·0002	0·0017	Nitrate of silver.
4	402·1	0·0004	0·0010	Hydrochloric acid.
5	225·0	0·0004	0·0018	Nitrate of silver.
6	462·0	0·0004	0·0009	Hydrochloric acid.
7	696·4	0·0014	0·0020	Nitrate of silver.
8	825·4	0·0007	0·0008	Hydrochloric acid.
9	700·4	0·0014	0·0020	Nitrate of silver.
10	747·2	0·0007	0·0009	Hydrochloric acid.
11	520·9	0·0011	0·0021	Nitrate of silver.
12	287·5	0·0003	0·0010	Hydrochloric acid.

If we assume that the amount of chloride of silver precipitated by nitrate of silver under the conditions described above is a correct measure of the solubility of the chloride, it appears from the above determinations that about two milligrams of chloride of silver are dissolved by each liter of boiling water, and further that only about one-half of the amount thus dissolved is precipitated by hydrochloric acid.

In making chlorine determinations it is a very common practice to wash with very hot water in order to secure the prompt settling of the chloride of silver or to wash away any occluded material, and it was the chief object of this investigation to determine the extent to which the solubility of the chloride in distilled water might effect the result. For this purpose we may make two series of determinations of the

chlorine in chloride of antimony; in both cases precipitating with nitrate of silver the chlorine from a solution of the chloride of antimony in tartaric acid and water with the usual precautions. But, while in the first series the precipitated chloride of silver was washed with boiling hot distilled water to about the $\frac{1}{100,000}$ according to Bunsen's scheme; in the second series although hot water was also used in washing, one decigram of nitrate of silver per liter was added to each successive portion of the wash-water poured upon the precipitate, until the last two portions, which were poured on cold. By this simple device the advantages of washing with hot water may be secured while its solvent action is prevented. The results are given in the following table.

First Series.

No.	Weight of $SbCl_3$ taken.	Weight of AgCl obtained.	Per cent of Cl calculated.
1	2·3856 gram.	4·4784 gram.	46·441
2	3·1300	5·8712	46·407
3	3·4207	6·4243	46·462
4	5·0031	9·3790	46·377
		Mean value,	46·422
		Max. diff. from mean,	0·047

Second Series.

No.	Weight of $SbCl_3$ taken.	Weight of AgCl taken.	Per cent of Cl calculated.
1	3·4059 gram.	6·4188 gram.	46·624
2	3·6603	6·9014	46·643
3	2·4762	4·6658	46·617
4	2·5567	4·8212	46·651
		Mean value,	46·634
		Max diff. from mean,	0·017
	Difference between means of two series,		0·212

It is evident from these results that when great accuracy is required the solubility of chloride of silver may become a very serious source of error in determinations of chlorine, and in our investigation * of the atomic weight of antimony this was the chief cause of the discrepancy between the analyses of chloride of antimony on the one hand and the bromide, iodide and sulphide of antimony—analyses of which closely agreed among themselves—on the other hand. It was shown in the paper just referred to that, although the greatest care was taken in purifying the material, the chloride of antimony used actually left behind on evaporation a sufficient amount of oxichloride to reduce the per cent of chlorine 0·116.† The mean

* This Journal, III, xv, 41. † This Journal, III, xv, 118.

results which we actually obtained from seventeen analyses of chloride of antimony was 46·620, and when to this we add 0·212 and 0·116 the sum is 46·948, which differs from 47·020—the theoretical value when Sb =120, and Cl =35·5—by only 0·072. In this estimate we leave out of the account the known solvent action on chloride of silver of the tartaric acid used to keep the antimony in solution. This must equally affect both of the series of determinations given above and fully accounts for the small difference that remains to be explained. This whole discussion, however, only serves to confirm the conclusion previously expressed, that chloride of antimony is a most unsuitable material for the basis of an atomic weight determination, and having explained the anomaly to which we gave prominence in a previous paper we shall here take leave of the subject. We have also studied the solubility of bromide of silver in water, but this is so small that we found it difficult to measure the amount. In water at the ordinary temperature, or even in tepid water, bromide of silver is practically insoluble. In boiling water it is perceptibly soluble but not more so than is chloride of silver in water at the ordinary temperature. Hence the determination of bromine does not require the same precautions and is susceptible of greater accuracy than that of chlorine; and on this account, as well as for other reasons which we have previously discussed, the atomic weight of antimony can be deduced from the analyses of the bromide of antimony with as great accuracy as can at present be reached in such determinations. In the seven determinations of the per cent of bromine in bromide of antimony which we have published,* the maximum difference from the mean value 66·6651 was only 0·0045, and Professor Mallet in his analyses of bromide of aluminum has obtained with the same method a similar degree of accuracy.†

In conclusion we would again express our obligations to Mr. G. M. Hyams, who has assisted us in the work of this investigation.

* This Journal, III, xix, 385.

† Philosophical Transactions, Part III, 1880, 1022.

ART. XXIX.—*Papers on Thermometry from the Winchester Observatory of Yale College;* by LEONARD WALDO.

[Continued from page 61.]

THE observation of the freezing point of "Kew 585" for Nov. 2d, is not entitled to the weight of the ones made Dec. 1st because it was made by the eye alone, while the readings for Dec. 1st depend on the cathetometer.

We may assume therefore that the constants of these standards for the present are as follows:

	Kew 578.	Kew 584.	Kew 585.
Correction at the permanent freezing point	0°·00	+0°·10 F.	0°·00
Correction at the boiling point	+0°·03 C.	+0°·20 F.	+0°·14 C.
Depression of the freezing point after an exposure to the boiling point	0°·18 C.	0°·25 F.	0°·22 C.

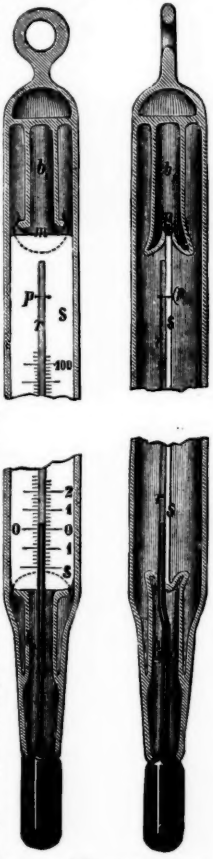
These are the constants which have been inserted in the formulæ for computing the corrections to the Kew thermometer readings. I shall now investigate the constants of the thermometers similar to the standards of the "Kaiserliche Normal-Eichungs-Kommission" and which have been carefully compared with these standards through the kindness of Dr. Fœrster in Berlin. These thermometers were made by R. Fuess, and I cannot better describe them than by giving their dimensions in the following table and then adding a free translation from the "Bericht über die wissenschaftlichen Instrumente auf der Berlin Gewerbeausstellung im Jahre, 1879," pages 213 and 214.

Designation.	R. Fuess, Berlin, 89.	R. Fuess, Berlin, 50.
How graduated	−6° to +105° C.	−2° to +105° C.
Length of 1°	4·15 mm	4·05 mm
Smallest graduation	0°·1	0°·2
Length of tube	650 mm	573 mm
Outside diameter of tube	12 mm	12 mm
Shape of bulb	Cylindrical.	Cylindrical.
Length	15 mm	12 mm
Diameter	5·5 mm	5·5 mm

* * * * * * *

"Fuess has succeeded in devising a construction for thermometers which is free from all those imperfections, and com-

bines great stiffness with elegance of form. Any displacement of the scale and all slipping or bending of the capillary tube is prevented, and it is possible for the outside tube, the capillary tube and the scale, each to expand independently of either of the others. The normal thermometers made by Fuess which Professor Wild of St. Petersburg describes* 'as the best' which he has 'up to this time become acquainted with,' were made according to this construction, which is protected by patent (D. R.—P. No. 389)."



The figure shows a thermometer of this kind. A hollow glass cup b_2, open at the top, is hermetically sealed to the so-called neck of the surrounding tube. The upper edge of the cup is deeply indented in two diametrically opposite places, so that the two indentations offer a sure bed for the scale plate *s*. To the glass top of the thermometer there is a similar fixed cup b_1, hermetically sealed, the indentations of which, diametrically opposite to each other, receive the upper end of the scale. To hold the scale in its place there is a spring placed between it and the cup b_1, which always presses the scale against its lower bed, without hindering its expansion. The capillary tube *r* passes freely through the hollow cup but is guided by loops *p* of platinum wire as fine as hairs, which are drawn through minute holes in the scale; so that without hindrance from the loops *p* it can freely expand but cannot bend."

Both of these standards are exquisite specimens of the glass-blower's skill, and reflect credit upon the artist who made them. They possess the great conveniences of this form of mounting, in being

* "Bericht über Art. 10 des Programmes der zweiten internationalen Meteorologen-Kongresses in Rom., St. Petersburg, 1878, S. 8. The remark there made that these thermometers are constructed 'partly according to Mr. Pernet's designs' has been universally misunderstood, and to the clearing up of this misapprehension, which has been given on another page, the writer hopes to contribute by means of the following statement derived from his own recollection.

At the end of the year 1876 there had a rivalry arisen between the members of the Normal-Eichungs-Kommission and the mechanics who made the last thermometers, concerning a sure fastening for the scales of normal thermometers. This happened especially in connection with the studies in thermometry which the writer had made in the autumn of 1876 at the South Kensington exhibition and

easily read and in the certainty with which the eye may be so placed that the readings have no sensible parallax. They seem to leave little to be desired unless it be calibrating chambers at the upper ends, in order that the observer may easily separate just the length of the mercury column he may wish for calibration. Still it is generally possible to separate a column of about the length desired, with a little patience.

The following is a resumè of the results obtained by Dr. Fœrster at Berlin in the determinations of the constants of these standards. These results are reduced to latitude 45°, a barometric pressure of 760mm. and to the level of the sea.

Date.	Thermometer.	Correction at the Freezing Point after exposure to 0° C.	Correction at the Boiling Point.	Correction at the Freezing Point after exposure to 100° C.
1880. June 4	Fuess 50	− 0°·163	----	----
8	"	− 0·170	----	----
8	"	----	− 0°·035	+ 0°·145
17	"	----	− 0·012	+ 0·135
17	"	----	− 0·011	+ 0·135
Sept. 17	"	− 0·18	----	----
June 4	Fuess 89	− 0·334	----	----
8	"	− 0·347	----	----
8	"	----	− 0·027	0·000
17	"	----	− 0·023	− 0·005
17	"	----	− 0·047	− 0·023
Sept. 17	"	− 0·37	----	----

And Dr. Fœrster further finds that the depression of the freezing point after an exposure to the boiling point is for

Fuess 50 = 0°·35 C.
Fuess 89 = 0°·32 C.

Dr. Fœrster made a careful determination of the corrections to be applied to the Fuess thermometers owing to the errors of calibration for each consecutive degree. These tabular corrections are computed for a freezing point of +0°·37 in the case of Fuess 89 and of +0°·18 in the case of Fuess 50.

from the report of which Professor Neumayer's account contains an extract. In the spring of 1877 a thermometer was injured, in the calibration and comparison of which much trouble had been taken, and Dr. Pernet, who was then active in the Commission, became quite interested in this question. He attempted to obtain a sure fastening for the scale by using various kinds of cement, and attempted, among other things, to obtain an unchangeable position of the capillary tube and the scale by making a small mark on the former, and fastening it opposite a small mark on the scale. About the same time Fuess conceived the new and striking idea, not to fasten the scale directly, but only to place it immovably in a firm resting place. Later he tried to carry out this idea without the use of any cement, by a construction made entirely of glass. The use of platinum threads as guides for the capillary tube on the other hand is an invention of Dr. Pernet." —*Dr. L. Loewenherz.*

After we had received these thermometers we made the following determinations of the freezing point corrections:

Date.	Fuess 50.	Fuess 89.
1880. Sept. 21	− 0°·20	− 0°·40
28		− 0·40
Oct. 16	− 0·20	− 0·40
28		− 0·45
Nov. 25	− 0·20	− 0·43

from which it will be seen that the thermometers are slowly increasing their freezing point readings with time. We may assume the following as the corrections at the dates given.

Date.	Fuess 50.	Fuess 89.	Kew 578.
1880. June	− 0°·17	− 0°·35	0°·00
Sept.	− 0·18	− 0·37	0·00
Nov.	− 0·20	− 0·41	0·00

The thermometer Fuess 50 had a part of its upper scale support (the front lip of *m* in the figure) broken in coming from Germany. This does not affect the accuracy of its readings, I think, but in order to obtain a comparison up to 40° C. between a Kew and a Fuess standard I have chosen the thermometer Fuess 89, which reads to 0°·1 and is in excellent condition, and the Kew standard 578, of which the errors are extremely small.

In the following comparison the readings have been freed from the zero and 100° point errors, as well as those arising

Date.	Fuess 89.	Kew 578.	F. 89 − K. 578.
1880. Sept. 28	10°·67 C.	10°·62 C.	+ 0°·05 C.
21	10·81	10·75	+ 0·06
28	16·24	16·09	+ 0·15
21	21·14	21·00	+ 0·14
28	21·36	21·20	+ 0·16
Oct. 28	21·80	21·64	+ 0·16
28	21·81	21·66	+ 0·15
Sept. 21	24·21	24·03	+ 0·18
28	26·60	26·45	+ 0·15
21	31·66	31·50	+ 0·16
28	32·17	32·00	+ 0·17
Oct. 28	32·67	32·50	+ 0·17
28	33·31	33·15	+ 0·16
Nov. 26	34·08	33·90	+ 0·18
26	34·21	34·00	+ 0·21?
Sept. 28	37·31	37·15	+ 0·16
28	42·40	42·25	+ 0·15

from imperfect calibration. Both standards had been kept for three months previously hanging in a room whose temperature never exceeded 30° C. The comparison therefore represents the comparison of a stem graduated Kew standard with a porcelain-scale-graduated normal of the German form, up to 40° C. It is, however, only preparatory to a more rigorous comparison of the scales of two Kew and two Fuess standards between 0° and 100° C.

It did not occur to me until after these thermometers had been heated that the discrepancy could be so great as it is between these standards, and therefore there are no comparisons between 0° and 10° recorded in our note books until after the thermometers had been exposed to 100° C. I shall recur to this point in the continuation of the paper.

[To be continued.]

ART. XXX.—*On the Determination of the Coefficient of Expansion of a Diffraction grating by means of the Spectrum;* by T. C. MENDENHALL, of Tokio, Japan.

IN all precise measurements of wave-lengths, a correction for variations in the temperature of the grating is very essential. If the ruling be upon glass the coefficient of expansion resulting from any of the many well known determinations may be accepted as sufficiently accurate, because it is not large and because the results do not differ much among themselves. Neither of these statements can be made, however, concerning a ruling upon metal. Many of the beautiful gratings produced within a few years upon Mr. L. M. Rutherfurd's engine, are upon an alloy of tin and copper. As far as I have been able to learn, the coefficient of expansion of this alloy has not previously been determined, and, not being able to make use of any other method, a series of experiments was made for the purpose of obtaining the desired result from the spectrum itself.

From the well-known equation

$$\lambda = s \sin b$$

in which λ represents the wave-length, s, the distance between the centers of two adjacent lines upon the grating and b the angle of deviation, we easily obtain the following:

$$\frac{\delta s}{s} = -\cot b \, \delta b$$

from which it appears that, in order to determine the co-

efficient of expansion, it is not necessary to know either the wave-length of the line upon which the measurements are made or the value of the grating space.

Unfortunately there are considerable difficulties in the way of the direct application of this formula. It is not easy to contrive any plan by means of which the temperature of the grating can be varied by any considerable and known amount, and, at the same time, to insure that these changes in temperature shall not be accompanied by even a slight shifting of the position of its plane in relation to the collimating telescope. To accomplish the first of these results, a box, measuring about 8 cm. long, 5 cm. wide, and 8 cm. deep, was cut out of a solid block of wood, and of this box the grating was made to serve as one of the longer sides. The box was made waterproof, and a thin layer of paint along the edges to which the grating was applied was sufficient to prevent leakage at that point. The grating was bound to the wooden box by two wires, running one about the upper and the other about the lower portion. This box was placed upon a thin slab of wood, of somewhat larger size, through which passed three levelling screws, and the whole was then placed upon the table of the spectrometer and properly adjusted. A similar box, made of metal, with the grating sealed in as one side, was first used, but it was promptly rejected on account of a tendency to shift its position slightly at each change of temperature. It is believed that the use of wood obviated this difficulty to a great extent. When adjusted so that its plane was over the center of motion, normal to the collimator, etc., the box was filled with water, in which was held a thermometer graduated to ·1° C., and capable of being read with considerable certainty to hundredths. The grating used was ruled with 8648 lines to the inch. The line set upon was one due to iron, wave-length about 5913, situated about 14′ or 15′ away from D, the spectrum being of the fourth order. The micrometers upon the reading microscopes were divided to single seconds, and the tenths were easily estimated. The set upon the line was made when the thermometer indicated something like a constant temperature, and then a portion of the water was removed and replaced by other at a higher or lower temperature as was desired. It was found desirable to confine the temperature within a moderate range. By working a few degrees above and below the temperature of the room, it was easier to secure constancy for a brief period at both points, and the thermometer would be much more likely to represent the real temperature of the grating. In addition to this, considerable changes in temperature were much more likely to be accompanied by a shifting of the plane of the grating.

Twenty measurements, in all, were made, the range of temperature varying from 5° C. to 16° C. From these the following result was obtained:

$$\delta b = 5''{\cdot}66 \pm 0''{\cdot}13.$$

The probable error is larger than would be desirable, and a good deal of irregularity in the results is to be attributed to the fact that we cannot be quite sure that the thermometer gave the true temperature of the grating, and also to slight shiftings in the plane of the grating, which doubtless sometimes occurred. Indeed, it is hoped and expected to secure a more accurate value by making a very exact determination of the position of the line in the winter, and then again in the summer when the difference in temperature may amount to as much as 15° C., and in this we shall be almost entirely free from both of the principal sources of error.

From the above value the coefficient of expansion is—

$$\varepsilon = {\cdot}0000202,$$

with a probable error of a little more than two per cent of the whole. As spectrum observations are generally made within a small range of temperature, this value is, perhaps, sufficiently accurate for their correction. I am unable to refer at present to any determinations of the coefficient of expansion of alloys similar to this which contains eight parts of tin to seventeen parts of copper. Most authorities agree fairly as to the coefficient for copper, but as to tin there is a considerable range—from ·0000217 to ·0000248. It is difficult, therefore, to determine what the coefficient for this alloy *ought* to be, but I venture the opinion that the above result is more likely to be too high than too low.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Density of Iodine Vapor.*—Shortly after Victor Meyer's discovery that at high temperatures iodine vapor had a density two-thirds of its normal value, Crafts and Meier studied the subject and found that this density decreased regularly from 650° to 1350°. Subsequently Meyer repeated his experiments, using Crafts' method of measuring the temperature, and found that at a very high temperature, the value approached one-half of the normal density. The numbers obtained in the two investigations differed considerably, however, especially at 1650°, at which point Meyer found 5·83 and Crafts and Meier 7·1. More recently Troost obtained 5·7 at 1250°; from which Meyer concluded that Dumas' method should give the same re-

sult as his own. But the influence of tension upon dissociation is so considerable, that Crafts and Meier undertook an investigation of the variation of the density of iodine vapor with the tension as well as the temperature. They have now given the results, some of them graphically in the form of curves, in which the ordinates represent densities, the abscissas temperatures. The densities are in all cases referred to air at the same temperature and pressure as unity. These plottings show (1) that up to 355° all the curves are contained in a single line parallel to the axis of abscissas, and the density is normal; (2) that with rising temperature, the variation is more rapid up to the middle of each curve, then it diminishes and becomes zero at the highest temperature, so that under low tensions the density becomes constant again at 1400° to 1520°, being then equal to one-half the normal value; and (3) that the curves which depart first from the straight line and which first unite again to form the high temperature line are those which represent the lowest tensions. These results explain the discrepancies before observed, and appear to confirm the hypothesis already advanced by the authors, that at low temperatures iodine exists molecularly as I_2 and at high temperatures atomically as I; and that the variations of density with temperature and pressure correspond to a progressive dissociation.—*C. R.*, xcii, 39, Jan., 1881. G. F. B.

2. *Researches upon Ozone.*—Hautefeuille and Chappuis have presented to the Chemical Society of Paris a valuable memoir upon ozone. They first studied the effect of temperature and pressure on the transformation of oxygen into ozone, using the silent discharge for this purpose, in the apparatus of Berthelot. At 760 mm. pressure 0·106 of ozone by weight was formed at 20°, 0·149 at 0° and 0·214 at −23°. Under 180 mm. pressure, the quantity was 0·089 at 20°, 0·137 at 0° and 0·181 at −23°. Next the effect of mixture with other gases was investigated, and it was found that while at 0° no effect was observed, at −23° the amount of ozone was increased to 0·216 when the oxygen was mixed with four times its volume of nitrogen, and to 0·240 when twice as much nitrogen was present. Hydrogen and silicon tetrafluoride increase still more the quantity of ozone. The authors then examined the physical properties of ozone, the density as determined by Soret, and the heat of formation by Berthelot being the only data heretofore known. The production of a gaseous mixture containing 21 per cent of ozone—which is permanent at −23°—enabled the authors to obtain interesting results on compressing it. The oxygen cooled by methyl chloride, was ozonized and placed in the compression tube of a Cailletet apparatus, care being taken to keep the temperature at −23°. On working the pump, the first few strokes developed an azure blue color in the tube, which deepened as the compression increased till under a pressure of several atmospheres, it became indigo-blue. Cooling the tube to −88° by immersion in liquid hyponitrous oxide, the color became three or four times darker.

Upon filling a tube a meter long with the ozonized oxygen at the ordinary pressure, and looking through it at a white surface, the gas was seen to have a sky-blue color, darker as the amount of ozone was greater; thus proving ozone to be a colored gas. The question next arose, is liquid ozone colored? By a sudden expansion from a pressure of 75 atmospheres at $-23°$, a thick mist was momentarily produced (this result not taking place with oxygen below 300 atmospheres). Compression to 200 atmospheres at a temperature of $-88°$ did not give on expansion any visible liquid. By adding, however, carbon dioxide gas whose point of liquefaction is not far from that of ozone, a liquid layer was obtained, blue in color and not differing in shade from the gas above it. When slightly expanded and then immediately compressed, an azure blue liquid is obtained much deeper in color than the gas, the cooling produced by the expansion reducing the temperature of the ozone below its critical point. Hence a mixture of oxygen and ozone containing above 50 per cent of the latter gas compressed below $-88°$ would give drops of a dark blue liquid. The mixture of ozone and oxygen should be compressed slowly with care and be well cooled; otherwise the ozone is decomposed with detonation. It must therefore be classed with explosives, since Berthelot has shown that 24 grams of oxygen absorbs 14·8 calories in conversion into ozone. Finally, the authors made use of this blue color of ozone to ascertain whether the products of the decomposition of carbon dioxide by the silent discharge, contained this gas. Upon compressing the products of the action of the electric discharge upon carbon dioxide in a Cailletet apparatus, not only was a colored gas obtained, but the undecomposed CO_2 liquefied and became blue.—*Bull. Soc. Ch.*, II, xxxv, 2, Jan., 1881. G. F. B.

3. *On the Chlor-hyponitric acid of Gay Lussac.*—Goldschmidt has submitted to examination the product obtained by Gay Lussac by distilling aqua regia, and called by him "acide hypochloronitrique." Three parts of hydrochloric acid of sp. gr. 1·16 was mixed with one part of nitric acid of sp. gr. 1·36 and submitted to distillation on the water bath. The evolved vapors were passed first through an empty flask, then through a chloride of calcium tube and finally condensed in small flasks, placed in a mixture of calcium chloride and ice, which were subsequently sealed. Weighing, before and after, gave the quantity of liquid in them. Breaking the flask under water decomposed the liquid and in the resulting solution the chlorine was determined and found to vary from 60·77 to 68·23 per cent, the formula $NOCl_2$ requiring 70·30 per cent. On fractioning this distillate, three portions were obtained containing respectively 58·89, 56·91, 56·39 per cent Cl, the formula NOCl requiring 54·2. The first distillate, therefore, was evidently a mixture. To determine whether the gases as evolved from the aqua regia contained nitrosyl chloride and chlorine, or nitrosyl chloride and chlorhyponitric acid, or all three, their vapor density was determined and found

to be 2·358, 2·385 and 2·350 in three experiments, the total chlorine present being 80·5, 81·6 and 78·9, per cent. Hence it cannot be a mixture of NOCl and $NOCl_2$. If a mixture of $NOCl_2$ and Cl, then from the above data there would be 3 molecules free Cl to 4 molecules $NOCl_2$ which would give a vapor density of 3·049. If a mixture of NOCl and Cl, it would contain 5 molecules free Cl to 4 molecules NOCl, giving a vapor density of 2·372 very near that actually found. The first distillate had a vapor density of 2·315, which taken in connection with its content of chlorine, 68·4 per cent, shows it to be a mixture of NOCl and Cl_2. Hence Gay Lussac's chlor-hyponitric acid is a mixture of nitrosyl chloride with a varying proportion of absorbed chlorine gas.—*Liebig's Ann.*, ccv, 372, Nov., 1880. G. F. B.

4. *On Mercuric Fulminate and its Decomposition.*—Berthelot and Vieille have studied the properties and conditions of decomposition of mercuric fulminate. Their material was that used in the service and it gave on analysis numbers agreeing with the formula $C_2N_2HgO_2$. Its decomposition products were determined by exploding about three grams of it electrically in a steel eprouvette, previously filled with pure dry nitrogen under known pressure and temperature. As a mean of five experiments, one gram gave 234·2 c. c. of gas, theory requiring 235·8. In 100 volumes this gas contained HCy and CO_2 0·15, CO 65·70, N 32·28 (Ratio 2·04 : 1) and H 1·87. Hence the reaction is: $C_2N_2HgO_2 = (CO)_2 + N_2 + Hg$. It appears, therefore, that no compound capable of dissociation is formed, and that consequently no gradual reunion takes place capable of moderating the expansion of the gas and of diminishing the violence of the initial shock. Hence the energy of the explosion. The heat produced was measured by placing the eprouvette in the above experiments in the water of a calorimeter. As a mean of the five experiments, one gram gave 403·5 gram-degrees; a quantity of heat which applied to the products of the detonation would heat them to nearly 4200°. From these data the heat of formation may be calculated: C_2 (diamond) $+ N_2 + O_2 + Hg$ (liquid) $= C_2N_2HgO_2$ absorbs $51·6 - 114·5 = -62·9$, which is negative as would be expected. Hence the heat resulting from its explosion has two sources: First, the separation of its elements; and second, the union of the carbon and oxygen to form CO. In contact with air, more heat is evolved because the CO burns to CO_2; but the force of the explosion is not increased thereby, since this combustion follows it. When mixed with potassium chlorate or nitrate, the amount of heat is doubled; but the initial force of the explosion is modified by the dissociation of the carbon dioxide, so that the total effect is less than before. The pressures developed by the explosion were measured in a special eprouvette called a "crusher," by means of the compression produced upon a cylinder of copper. With 2·43 grams fulminate, the pressure developed was 477 kilograms per square centimeter. With 4·86 grams, 1730 kilos. With 7·39 grams, 2697 kilos. And with 9·72 grams, 4272 kilo-

grams. Hence, though sudden, the pressure produced by the explosion is not very great; that produced by gun cotton being nearly twice as great, and that produced by dynamite, containing 75 per cent of nitroglycerin, being about the same. The effectiveness of the fulminate arises from (1) the instantaneousness of its decomposition, (2)the absence of dissociation products, and (3) its great density, 4·42. It develops at the point of contact an enormous instantaneous pressure which has no relation to the mean pressure in the vessel. This pressure Berthelot estimates at 48,000 atmospheres, while gun cotton develops on contact only 24,000 atmospheres.—*Ann. Chim. Phys.*, V, xxi, 564, Dec. 1880.

G. F. B.

5. *On the Direct production of Chloroform and Bromoform.*—Damoiseau has studied the action of porous bodies in causing the reaction of chlorine upon methyl chloride. A regular current of chlorine gas mixed suitably with methyl chloride is passed through a long tube filled with animal charcoal and heated to 250°−350°. By washing with water the hydrochloric acid gas is removed, and the condensed product corresponds to the mixture employed. Chloroform is produced with great ease and uniformity in this way. Bromine acts similarly, producing from CH_3Br, CH_2Br_2, $CHBr_3$ and CBr_4. Acetic acid treated with 2, 4, 6 atoms of bromine in this way, gives carbon dioxide and brominated derivatives of formene. Chlorine and acetic acid gives chloroform in considerable quantity.—*C. R.*, xcii, 42, Jan. 1881.

G. F. B.

6. *On the Identity of Arabinose with Lactose.*—Kiliani has examined the sugar obtained by Scheibler by the action of dilute sulphuric acid upon gum arabic, and which he called arabinose. One part of pure gum arabic was boiled for 18 hours with 8 parts of a two per cent solution of sulphuric acid in an open dish, the evaporated water being replaced. The acid was then removed by barium hydrate, the filtrate evaporated to a syrup, agitated with alcohol, decanted, the alcohol distilled off, and the syrup allowed to crystallize over sulphuric acid. On analysis, it gave the formula $C_6H_{12}O_6$; it rotates the polarized ray $[\alpha]_D = 79°$, reduces Fehling's solution, ferments with yeast, yields mucic acid when oxidized with nitric acid, and dulcite when reduced with sodium. For these reasons, the author thinks the arabinose of Scheibler is identical with milk sugar or lactose.—*Ber. Berl. Chem. Ges.*, xiii, 2304, Jan. 1881.

G. F. B.

7. *On the absorption of dark Heat rays by Gases and Vapors.*—Herr Ernst Lecher and Herr Joseph Pernter criticise the results of previous observers, especially those of Tyndall and Magnus. Taking Tyndall's numbers, they show that the absorption of radiant heat is different when a polished metallic tube is employed instead of partly blackened tubes or tubes of glass, and therefore point out that Tyndall's results are subject to an error. They show, also, that vapor adhesion introduces another source of error in Tyndall's work. In order to avoid all possible errors

the author placed a thermopile as well as the radiating surface inside the experimental space. The experimental vessel consisted of an inverted glass bell jar with the open end placed upward. The thermopile with one face placed upward and toward the source of radiation and the other carefully protected by a packing of cotton-wool, was supported at the bottom of this jar. The entire jar and the cover was surrounded by water which was kept running from a tap. The radiating surface at the mouth of the bell jar was heated by a jet of steam, and the details of the experiment were so arranged that equal quantities of heat were communicated to the plate in equal times. The copper wire of the galvanometer was wound with white silk in order to avoid any trace of iron, and it was protected from sudden changes of temperature. No binding screws were employed. The wires leading from the galvanometer were carefully wound with those of the external circuit, and the junctions having been varnished were placed in running water so as to avoid the production of thermo-electric currents. One of the two conducting wires was completely insulated, for the authors confirmed the earlier result of Lamont, that perfectly dry wood is not an insulator for feeble currents. The experiments with pure dry air made it apparent that the absorption of heat rays from a source of heat at 100° C. in passing through a layer of air thirty-one centimeters in thickness is so small that it cannot be measured. From experiment it was found that out of 100 incident rays 99·78 pass through. With moist air no absorption was found. This result was surprising, since the well-known absorption of heat rays of the sun by the atmosphere has generally been attributed to aqueous vapor. After a general discussion of the meteorological bearing of these results it is concluded that the question of the amount of absorption of radiation by our atmosphere can not be solved at present; and it will be necessary in order to obtain a true solution to arrive at the proportion of carbonic acid in the air and also the amount of vegetable and animal organisms. It is pointed out that an accurate solution can also only be obtained when the absorption is known for the different parts of the spectrum.—*Sitzb. der K. Akad. der Wissensch. in Wien*, July, 1880; *Phil. Mag.*, Jan. 1881. J. T.

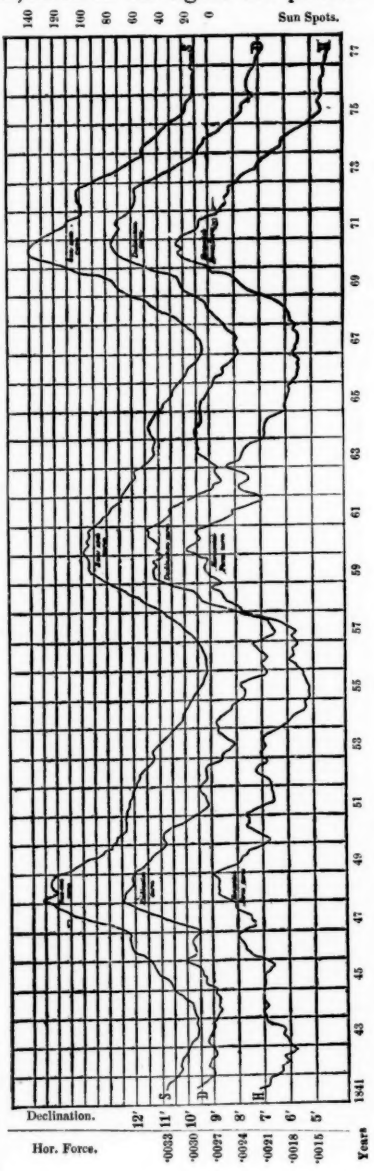
8. *Dust, Fogs, and Clouds.*—Mr. JOHN AITKEN, in a paper presented to the Royal Society of Edinburgh, maintains the theory that dust is essential for the formation of fogs and clouds. Steam was mixed with air in two large glass receivers; one of these receivers was filled with common air, the other with air which had been carefully passed through cotton-wool so that all dust might be removed. In the one receiver the air presented the usual cloudy appearance on the entrance of the steam, while in the other no cloudiness was apparent, the air remaining supersaturated and perfectly transparent. In another experiment water was placed beneath a receiver and the temperature of the enclosed air lowered by reducing its tension. With unfiltered air the

receiver was immediately filled with a dense vapor, with filtered air, however, no cloudiness was perceived. It was concluded from these experiments: (1) Whenever water vapor condenses in the atmosphere it always does so on some solid nucleus; (2) that dust particles in the air form the nuclei on which the vapor condenses; (3) that if there was no dust there would be no fogs, no clouds, no mists, and probably no rain, and that the supersaturated air would convert every object on the surface of the earth into a condenser on which it would deposit; (4) our breath when it becomes visible on a cold morning and every puff of steam shows the impure and dusty condition of the atmosphere. Among the sources of atmospheric dust is the spray of the ocean. By simply heating a piece of metal—the one hundredth of a grain of iron wire—the dust driven off will give a cloudiness in the experimental receiver. Common salt burned in an alcohol lamp gave an intense fog, and burned sulphur gave a fog so intense that it was impossible to see through a thickness of 5 cm. of it. It is calculated that more than 200 tons of sulphur are burned with the coal every winter's day in London, and this quantity is sufficient, in the writer's opinion, to account for London fogs.—*Nature*, Dec. 30, 1880. J. T.

9. *On the relation between the Diurnal Range of the Magnetic Declination and Horizontal force, as observed from* 1841 *to* 1877 *at the Royal Observatory, Greenwich, and the period of Solar Spot frequency;* by WILLIAM ELLIS, F.R.A.S.—Observations of the magnetic elements have been carried on at Greenwich since 1841. Until 1847 actual readings of the various instruments were taken at two-hour intervals, but since the beginning of 1848, in addition to the absolute determinations of declination, intensity and dip, a continuous record of the variations of the declination and the horizontal and vertical components of the total force has been obtained by the method of photographic registration, devised by Mr. Charles Brooke. The fact that during this long period the observations of the declination and horizontal force, discussed in this paper, have been made on the same plan and with the same instruments, gives especial value to the conclusions which are drawn from them.

The mean diurnal range of declination in each month is taken to represent, relatively to other months, the magnetic energy of the month; and similarly for the horizontal force. This mean diurnal range for each month is obtained by taking the difference between the greatest and least of the mean indications for each separate hour through the month, days of great magnetic disturbance being rejected. A table is thus formed giving two series of numbers: the variation in declination is expressed in minutes of arc; for the variation of the horizontal force the unit is ·0001 of the whole horizontal force. The numbers thus obtained show a marked increase for the summer months. This annual inequality having been, by special calculation, eliminated, a second table is formed giving the annual mean of the monthly

mean diurnal range of the declination and horizontal force. From this table the curves *D D*, *H H* in the figure are plotted (the original plate, one of three illustrating the memoir, has been here reduced to one-third by photography.) The scales for the curves of declination (*D D*) and horizontal force (*H H*) are so arranged that each minute of arc of declination is represented by ·0003 of horizontal force, and so on in proportion. The third curve (*S S*), that of sun-spot frequency, is constructed by taking one minute of declination to correspond to 20·0 in sun-spot number.



A comparison of the three curves shows strikingly the correctness of the view generally held, that *the diurnal ranges of declination and horizontal force are subject to a periodical variation corresponding to the* 11*-year sun-spot period.*

Other conclusions deduced by Mr. Ellis are as follows: (1.) That the epochs of minimum and maximum magnetic and sun-spot effect are nearly coincident, the former occurring on the whole somewhat later. The variations of duration in different periods appear to be similar for both phenomena.

(2.) The occasional more sudden outbursts of magnetic and sun-spot energy, extending sometimes over several months, appear to occur nearly simultaneously and progress collaterally.

(3.) It seems probable that the annual inequalities of magnetic diurnal range, are subject also to periodical variation, being increased at the time of a sun-spot maximum,

when the diurnal range is increased, and diminished at the time of a sun-spot minimum, when the mean diurnal range is diminished.

The evidence in favor of this final conclusion is considered as not entirely decisive; the other conclusions are regarded as sufficiently certain.

10. *U. S. Coast and Geodetic Survey*, CARLILE P. PATTERSON, Superintendent in charge. *Methods and Results: American Standards of Length* (Appendix, No. 12—Report for 1877). 36 pp. 4to. Washington, 1880.—This memoir contains the report by Professor J. E. Hilgard on the details of observations made to establish the relation between the American and British standards of length. The general results reached (published in Appendix 22, Report for 1876), show—(1) That there is no difference between the standards of weight of Great Britain and those of the United States; (2) This is also true of the standards of length; (3) The standards of volume in the United States are the same as those lawful in Great Britain prior to 1826. Also, (4) the relation of American and British standards to the French metric standard is not determined with extreme precision, but the legal enactments will suffice for all purposes except those of great scientific accuracy.

The standards of length described in the report are the Troughton 86-inch scale and the British standards, the bronze yard, No. 11, and the iron yard, No. 57. The Troughton scale (London, 1814), is a bronze bar, 86 inches long, $2\frac{1}{2}$ inches wide, and $\frac{1}{2}$ inch thick, it has an inlaid silver scale; it was made for the survey of the coast of the United States. The yard of 36 inches, between the 27th and 63d inch was adopted as the standard yard of the United States by the Treasury Department, in 1832. The two other standards were presented by the British Government in 1856.

A detailed discussion is given, in the report, of the coefficients of expansion of the three standards, and of the results of comparisons between them, and also of each of them with the Imperial yard and other British standards. As the result of this it is found that the Troughton scale is equal to the standard British yard at 59°·62 F., the bronze yard, No. 11, at 62°·25 F., and the iron yard, No. 57, at 62°·10 F. Furthermore it is stated that between 1856 and 1872 both the last two standards, for want of proper provision for their safe keeping, have been subjected to great variations of temperature, varying fully 75° F. As a result of this it is concluded that the bronze yard has shortened relatively to similar measures which have been kept at a nearly constant temperature. This standard was made, on account of its great rigidity, of Baily's metal consisting of 16 parts copper, $2\frac{1}{2}$ parts tin, and 1 part zinc, and it is suggested that the molecules of such a casting are probably in a state of great tension, likely to yield under changes of temperature, and perhaps in less degree to the simple effect of continuance. The wrought-iron bar, No. 57,

Low Moor iron, on the other hand, which has been subjected to the same vicissitudes as the bronze bar before mentioned, has maintained its relative length to the Imperial standard. As bearing upon the persistence of material reduced to the plastic condition, the fact is mentioned that in 1867 one of the original meters, long in the possession of the U. S. Coast Survey, was compared with the platinum meter of the Conservatoire des Arts et Métiers, made about 1800, and no difference of so much as a thousandth part of a millimeter after so many years was detected, although it had been twice transported across the ocean and had experienced extreme vicissitudes of temperature.

11. *Magnetic Declination in Missouri.*—A chart has been issued by Professor Francis E. Nipher, of St. Louis, giving the declination of the magnetic needle in Missouri for 1879, and with the lines of equal declination deduced from the observations. The map is published for use of surveyors, but it is interesting as showing the remarkable irregularity in these isogonic lines. For example, immediately west of St. Louis the lines have a sharp bend to the westward, while on the same parallel toward the center of the State there is a correspondingly sharp bend to the eastward. (See this Journal, xix, 234, 1880).

II. Geology and Mineralogy.

1. *Pennsylvania Geological Survey: The Geology of McKean County, and its connection with that of Cameron, Elk and Forest;* by Charles A. Ashburner. Report of Progress, No. R. 372 pp. 8vo. With 33 plates and 2 maps. Harrisburg: 1880.—McKean, one of the northern counties of Western Pennsylvania, adjoins Cattaraugus County and a part of Allegany County of New York, and its geological and topographical relations to the latter region give the facts which Mr. Ashburner has carefully studied out special importance. It is further, the location of the northern oil area of the State—the Bradford. Mr. Ashburner's report treats first of the topographical features and surface geology; next, of the general geological structure; thirdly, of the economic products of the county; and then, in Part II, he gives the detailed geology of the county, under which are brought out many sections illustrating the Bradford oil district.

The region is part of the elevated plateau that stretches west and northwest from the Appalachians, and apparently makes, along with that of Southern New York and the Cumberland Table land to the south, the northwestern flank of the mountain range. The surface has "a mean height of nearly 2000 feet above the ocean," but many elevations over it reach to 2200 feet, and one to 2500.

The rocks at the outcrops—which are not numerous or good—are so nearly horizontal that what there are of anticlinals and synclinals consist mostly of slightly curving "domes and dim-

ples," the latter only occasionally taking the form of a trough. The strata are a continuation of those of the Appalachian region to the southeast, but all the formations are greatly reduced in thickness. They belong to the following groups, commencing below:

A. DEVONIAN.—(1) The *Chemung* sandstones and shales (VIII) which include the productive "oil-sands" of the Bradford district;* (2) the *Catskill* sandstone (IX), 250 feet thick at Bradford (while 2560 feet in Blair County, just southeast of the center of Pennsylvania, and 5000 to 6000 feet at the extreme eastern outcrops on the Lehigh and Susquehanna Rivers).

B. SUBCARBONIFEROUS.—(3) The *Pocono sandstone* (X), 250 feet in average thickness (while 2133 feet in Broad Top Mountain, in Huntingdon County, east of Blair County) having, at middle, the Sub-Olean conglomerate, 40 feet thick; and overlaid by (4) the *Mauch Chunk Red shale* (XI), only 10 feet thick (while 3000 feet at Mauch Chunk in Carbon County); the two together correspond to the Subcarboniferous.

C. CARBONIFEROUS.—(5) The *Pottsville conglomerate*, or the so-called *Millstone Grit* (XII), 160 feet thick (while 1000 feet at Pottsville, in Schuylkill County), which consists, at bottom, of the Olean conglomerate (the rock which, on account of its tower-like and other bold features, suggested the name of Rock City to a place in Cattaraugus County, N. Y., and which is the same as the Sharon conglomerate of Western Pennsylvania and the conglomerate of the Ohio geologists), and, above this, of two sandstone strata with the *Alton Coal-bed* between. Next (6) The *Lower Productive Coal-measures* (XIII) 140 feet thick, which contain the *Dagus Coal-bed* and the *Clermont Coal-bed*, and are the most northern outliers of the Appalachian coal formation. The Clermont coal-bed (3 feet thick) is the same as the Clarion coal-bed of Western Pennsylvania, and the Dagus, situated above the "Clermont ferriferous limestone (4 to 8 feet thick) and some beds of sandstone and shale, is the Lower Kittaning.†

The Bradford Oil District is described geologically and economically, and illustrated by excellent maps and sections. It is elongated in the direction northeast-and-southwest. The length of the principal area is about sixteen miles (the northern two to three miles of it in the State of New York), and the greatest breadth is nearly eight miles. In its line, six miles to the southwest, lies the small Kinzua oil district, and half way between the two the "Big Shanty" district and toward the southern boundary of the county there are a few wells near Smethport and

* The "oil-sands" (or sandstones) of Western Pennsylvania, the Venango oil-district—are beds in the Catskill sandstone (IX) and thus have a higher position stratigraphically than those of the Bradford oil-district.

† With regard to the total thickness of the Paleozoic of Pennsylvania, Mr. Ashburner states that 40,000 feet, or about eight miles, "is certainly not excessive" in view of the fact that a section from the top of the Lower Productive Coal to the base (?) of the Trenton, carefully measured by him, showed a thickness of 3½ miles.

Kane. Thus the trend of the whole is approximately parallel to the course of the Appalachians. The depth of the wells is in nearly all cases 1100 feet, the borings extending from 40 to 150 feet below the top of the Chemung. To the south the depth is 1700 to 2000 feet; this is owing partly to a thickening southward of the overlying strata, but, about Smethport, to the oil-bearing sand-bed being at a lower level in the Chemung by nearly 300 feet.

The Bradford oil district has been worked less than ten years. The production of oil amounted to but seventy-five barrels a day in 1874, but was 58,000 a day in the year ending June, 1880. Of the 2539 wells sunk in 1879, only 3 per cent were dry holes or unproductive; and of the 6249 between the beginning of 1875 and January, 1880, only 3·77 per cent.

The special peculiarities of the oil-bearing sand-rock of the Bradford district have already been mentioned in this Journal, (xix, 415, 1880) from a paper by Mr. Ashburner, and also the phenomena of the remarkable Kane Geyser well (xviii, 394) on the southern borders of the county, and the Wilcox Spouting well (xvi, 144, 1878) about three miles to the east of the Kane.

2. *Report of Progress of the Geological Survey of Canada, for* 1878–79. ALFRED R. C. SELWYN, Director. 8vo. Montreal, 1880. (Dawson Brothers).—This volume contains the following reports: Introduction, by Mr. SELWYN (6 pp.); on the Queen Charlotte Islands, by Mr. G. M. DAWSON; on explorations of the Churchill and Nelson Rivers, etc., by ROBERT BELL; on the Geology of Southern New Brunswick, by Messrs. BAILEY and ELLS; and Chemical contributions, by C. HOFFMANN. Mr. Dawson's report describes, and illustrates on colored maps, the distribution of Triassic and Cretaceous rocks over the Queen Charlotte Islands.

The map (for which only approximate correctness is claimed), makes the southern half of the islands, between the parallels 52° and 53°, Triassic (Alpine Trias, it containing *Monotis subcircularis* Gabb and *Halobia Lommeli* Wiss., etc.); a middle portion, about Skidegat Inlet, crossing the island obliquely, and a northern angle (north of the parallel of 54°), of Graham Island, Cretaceous; and the rest of Graham Island, Tertiary and "probably" Miocene. The Cretaceous rocks are stated to have *a thickness of* 13,000 *feet.* They are generally much flexed, and toward the head of Skidegat Inlet there is a very sharp and steep flexure bending double a coal-bed; and the coal is *anthracite.* Cretaceous fossils (including Ammonites, Belemnites, etc.) are numerous, and some of them have been described by Mr. Whiteaves in Mesozoic Fossils, Vol. I, Part 1. The Cretaceous and Triassic rocks are unconformable, and so also the Cretaceous and Tertiary. The close of the Tertiary was a time of igneous eruptions, as well as the Tertiary itself.

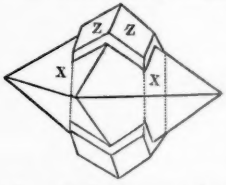
3. *A Monograph of the Silurian Fossils of the Girvan District in Ayrshire,* with special reference to those in the "Gray *collection;*" by Dr. H. ALLEYNE NICHOLSON and R. ETHERIDGE, Jr.

Fasciulus III.—This concluding part of vol. i contains pp. 237 to 334 (8vo), of the text and plates xvi to xxiv, and treats of the Annelids and Echinoderms, with supplements on the Protozoans, Cœlenterates, and Crustaceans. The first three of the excellent plates are devoted to fossil corals of the genera Heliolites, Plasmopora, Propora, Pinacopora and Halysites.

4. *Revue de Géologie pour les Années*, 1877 et 1878, par M. DELESSE et M. de LAPPARENT, tome xvi, 268 pp. 8vo. 1880. (Librarie F. Savy).—This volume of Delesse's Geological Annual, like its predecessors, gives a valuable résumé of geological papers, especially those relating to Lithological, Dynamical, Historical or Stratigraphical, Geographical and Agrinomic Geology. The abstracts are well prepared and in general quite full.

5. *Annelid Jaws from the Wenlock and Ludlow formations of the west of England.*—Several species of these annelid jaws are described and figured by Mr. G. J. HINDE, in the Quarterly Journal of the Geological Society for August, 1880 (p. 368).

6. *Xenotime, from Burke County, N. C.;* by W. E. HIDDEN. (Communicated.)—Symmetrically compounded crystals of xenotime and zircon, much like those first noticed by E. Zschau (this Journal, II, xx, 273) have been lately discovered by the writer in the auriferous gravels of Brindletown, Burke county, N. C. The accompanying figure shows the form to be somewhat different from those of Zschau, from Hitteröe, Norway; but the occurring planes are the same. The Burke county crystals are compounds of a *light-brown* zircon, with a *yellowish-gray* xenotime. The crystals are sometimes $\frac{1}{4}$ inch in diameter, though rarely; they are oftener $\frac{1}{10}$ inch through. About one in fifty of the xenotimes from this locality are thus compounded.



7. *Geological Charts of the Yellowstone Park, and of the region adjoining on the south in the territories of Wyoming, Idaho and Utah.*—Three large and beautifully colored geological charts have recently been issued as part of the results of the Geological and Geographical Survey of the Territories under Dr. F. V. Hayden for the years 1877 and 1878. The topography is given by contour lines. One chart (the most northern) is of the Yellowstone Park; the second, is a chart of parts of Western Wyoming and Southeastern Idaho; the third, of an adjoining part, to the south, of Wyoming and Idaho and part of Northeastern Utah. The area comprised by the three is nearly all that included between the parallels of 45° and 41° 45′, and the meridians, 100° 30′ and 112°. The first of these charts is on a scale of two miles to an inch and the others on that of four miles to an inch.

III. Botany and Zoology.

1. *The Power of Movement in Plants;* by Charles Darwin, LL.D., F.R.S., assisted by Francis Darwin. With illustrations. (London: John Murray. 1880. Appleton & Co., N. Y.) pp. 592. 18mo.—First let us congratulate the scientific community, no less than the author, that Mr. Darwin's experimental researches are seconded, and are we hope long to be continued, by the son whose name appears upon the title-page, and whose independent papers already published approve his worthiness for that honor. This volume is, from beginning to end, the record of a series of researches and of the inferences which they directly warrant. Naturally it will not fascinate the general reader after the manner of "The Origin of Species" and some of the volumes which succeeded that epoch-marking production; nor has it the fresh charms of the treatises "On the Movements and Habits of Climbing Plants," and of "Insectivorous Plants," of which it is the proper continuation and supplement.

The organs of plants take certain determinate positions and execute certain movements, some of them universal or general, some of them special, some of them very striking and seemingly strange, most—but not quite all of them—evidently advantageous to the plant or essential to its well-being. Roots point toward the earth; stems point away from it; young stems bend towards the light, and the upper face of leaves is presented to it. Stems that twine "*circumnutate*" (a capital term), i. e. bend successively to all points of the compass, and this wholly irrespective of external influences; and the twining around a support is a direct consequence of the circumnutation. Most tendrils freely circumnutate, and thereby are enabled to reach the object which they grasp. Most tendrils (and in certain cases some other parts) are very obviously sensitive to external contact or irritation, to which they respond by movement and change of form, and thus they grasp or do other advantageous acts. Some movements, especially of leaves, occur with regularity upon the access of light, others upon its withdrawal; a few, such as of the small leaflets of *Desmodium gyrans*, proceed irrespective of night and day. The specification need not be extended. The general facts in all their great variety are familiar to scientific readers. The inquiry of this volume is as to their ground and origin, or, as in this connection we should rather say, their development and history. For instance, circumnutation gives rise to twining and gives efficiency to other ways of climbing. But Darwin is bound to suspect, and even to show, that circumnutation is not a special endowment of the stems and tendrils of climbing plants, but rather a more developed manifestation of a general faculty. And the same is to be said of the movements of tendrils and leaves, or their appendages, whether automatic or in response to external irritation or stimulus. All this is what the experimental researches detailed in this volume undertake to ascertain and have satisfactorily made out.

An abstract of the volume might be somewhat tedious, and is certainly unnecessary for biological readers, who are sure to possess and study it. But the *gist* is readily to be gathered, without running through the iterated details or scanning many of the illustrative and curious figures which record the movements under investigation, by the simple perusal of the introduction and of the concluding chapter, in which the matter of the volume is summed up.

The sum and substance of the case is, that all these powers and faculties are manifested in the seedling immediately upon germination, and most of them are then remarkably exemplified. The caulicle or initial portion of stem below the cotyledons (with the elongation and protrusion of which the germination of dicotyledonous seeds usually begins) circumnutates as soon as it comes out into the open air, and even earlier: this is the earliest manifestation of an automatic movement which is shared by all the succeeding portions of stem developed from it, in the early life of most plants whether climbers or not. In the latter, and especially in twining plants, we see this general faculty at its maximum and in beneficial exercise. More remarkable and novel it is to learn that the initial root, growing from the lower end of the caulicle (not inaptly called by Darwin the *hypocotyl*) also shares in this faculty of circumnutation. As it penetrates the soil in its downward course, it cannot largely manifest this faculty, and indeed its power of circumnutation is always small; "but the circumnutating movement will facilitate the tip entering any lateral or oblique fissure in the earth or a burrow made by an earth-worm or larva; and it is certain that roots often run down the old burrows of worms. The tip, however, in endeavoring to circumnutate will [successively] press against the earth on all sides, and this can hardly fail to be of the highest importance to the plant" (being supplemented by another faculty, that of sensitiveness at the tip presently to be mentioned); for "when the tip encounters a stone or other obstacle in the ground, or even earth on one side more compact than on the other, the root will bend away as much as it can from the obstacle or the more resisting earth, and will thus follow with unerring skill the line of least resistance." Then, beside the almost universal heliotropic movement, by which each leaf or leaflet presents its superior surface to the direction of the greater light, Mr. Darwin shows that these organs also circumnutate, beginning even with the cotyledons or seed-leaves; although their sweeps generally form so narrow an ellipse that they move up and down in nearly the same vertical plane, a movement describing a circle being converted into one up and down.

These circumnutatory movements are of the most fundamental and therefore mysterious character. Although most commonly connected with growth, they are at bottom independent of it. This—contrary to some German physiologists—we must conclude from both DeVries' and Darwin's investigations. They are pro-

duced by the changing turgescence of the cells on different sides of a stem or footstalk, which may or may not be fixed by consequent growth or solidification. This Mr. Darwin, we presume rightly, concludes to be the faculty or susceptibility upon which heliotropism, geotropism and the like (not to speak of *apheliotropism*, *apogeotropism*, *paraheliotropism*, *diaheliotropism*, *hyponasty*, *nyctotropism*, and other terms which the incautious student may take to be powers instead of abbreviated expressions)—in other words, upon which the solar rays and some occult influence of the earth—act, modifying the sweeps or converting them into forth and back or other special movements. Among these, that which has been termed the sleep of leaves, better and briefly designated by the word nyctotropism, is thoroughly investigated in this volume, is shown to be far more general than has been supposed; and the conclusion is that the end subserved is a needful protection of the surfaces, mainly the superior surface, against cold from nocturnal radiation. *A priori*, looking at the structure of the leaf, one would have thought that the under surface had the greater need of such protection.

Not only are all these movements incipient in the seedling, but some of them are manifested more rapidly and extensively than in most mature plants. This should needs be, since, as Mr. Darwin states it, "Seedlings are subjected to a severe struggle for life, and it appears to be highly important to them that they should adapt themselves as quickly and as perfectly as possible to their conditions." Very properly, therefore, no small part of this volume is devoted to the seedling and to the behavior of its several parts. The most novel and unexpected results relate to the young root. Judging from its simplicity and from the medium in which it is developed, one would not look there for the endowments which Mr. Darwin finds in it. But this root-tip and the vegetable cells which compose it conspire to teach us that most simple structures may be wonderfully gifted. The tiny root exhibits three kinds of movement; first that of circumnutation, in which, endeavoring to bend in all directions its tip "will press on all sides, and thus be able to discriminate between the harder and softer adjoining surfaces, . . . and to bend from the harder soil and follow the lines of least resistance," so modifying advantageously its course from that to which geotropism constantly tends to give it. Moreover, the growing end of the root is sensitive to contact, and in a complex manner. If pressed above the tip, it bends there toward or around the impinging body, much as the end of a tendril bends around a support: thus it may follow, as roots do, along the unequal surface of a solid body. But, thirdly, if the tip itself be locally pressed, it exhibits different and more surprising sensitiveness, for it transmits an influence to an upper adjoining part, causing it to bend away from the affected side. This sensitiveness to contact is confined to little more than one millimeter of the tip; the part which bends is 6 or 7 or even 12 millimeters above. So, when the sen-

sitive tip in its downward growth strikes obliquely upon a stone or other obstacle, the part above at this distance, to which some influence must be transmitted, bends and carries the point away from the obstacle. Yet later, whenever a new portion of the side impinges upon the stone or other body, it will bend at that part toward instead of away from it, and so follow along its surface. It is the tip, likewise, which can discern that the air is moister on one side than on the other, and which thence "transmits an influence to the upper adjoining part, which bends toward the source of moisture." It is the tip only which is sensitive to gravitation. Well may Mr. Darwin affirm that there is no structure in plants more wonderful, as far as its functions are concerned, than the tip of the radicle. Also, that, "it is impossible not to be struck with the resemblance between the foregoing movements of plants and many of the actions performed unconsciously by the lower animals." "But the most striking resemblance is the localization of their sensitiveness and the transmission of an influence to an excited part which consequently moves. Yet plants do not of course possess nerves or a central nervous system; and we may infer that with animals such structures serve only for the more perfect transmission of impressions and for the more complete intercommunication of the several parts." The closing sentence of the book may be appended to this. "It is hardly an exaggeration to say that the tip of the radicle, thus endowed, and having the power of directing the movements of the adjoining parts, acts like the brain of one of the lower animals."

The movements "excited by light and gravitation," as well as the nyctotropic or sleep-movements so-called, are (as we have already stated) all referred by Mr. Darwin to modified circumnutation, "which is omnipresent whilst growth lasts, and after growth has ceased whenever pulvini are present," as in several classes of leaves. As respects the relation of external agents to the movements, note Mr. Darwin's remark: "When we speak of modified circumnutation we mean that light, or the alternations of light and darkness, gravitation, slight pressure or other irritants and certain innate or constitutional states of the plant, do not directly cause the movement; they merely lead to a temporary increase or diminution of those spontaneous changes in the turgescence of the cells which are already in progress."

Certain parts of plants turn or grow earthward. When this is attributed to gravitation, as it commonly is, the physicists have opportunity to complain of a misuse of the term. Although Mr. Darwin, like other writers, speaks of the influence of light and of gravitation in the same breath, without discrimination, we note with satisfaction his disagreement with those who "look at the bending of a radicle towards the center of the earth as the direct result of gravitation," and note especially the concluding *dictum*. "Gravity does not appear to act in a more direct manner on a radicle than it does on any lowly organized animal, which moves away when it feels some weight or pressure."

Why, we would ask, need the word *gravity* or *gravitation* be used at all in this connection?

The introduction to this volume contains a short article upon the terminology which is adopted in it, chiefly as regards such words as *epinasty* and *hyponasty*, geotropism and related terms, which it is most convenient to employ, and also the names of the several parts of the embryo and seedling. This is, we believe, almost the first English book in which the axial part of the dicotyledonous embryo below the cotyledons (the *radicle* of the systematic botanists even of the present day) is distinctly recognized as hypocotyledonous or initial stem, although on the continent and in America this has long been taught and accepted. None the less so although the term *radicle* has been retained for it (until recently by the present writer, at least), in order not to break with the terminology of systematic works. Mr. Darwin, in this volume, shortens the expression of "hypocotyledonous stem" into the term *hypocotyl*,—a fairly good English term, certainly better than the French *tigelle*. The objection to both is that the words will not take a substantive Latin form, as all such terms should. Wherefore the better name—an old one which we have reverted to in the last edition of the Botanical Text-book (Structural Botany)—is *caulicle* or *cauliculus*. The initial root, which grows from the lower end of the caulicle (or "hypocotyl") Mr. Darwin calls the radicle, following in this the ordinary English usage, except in very definitely distinguishing it from the cauline part above it. Being simply *root*, we have preferred uniformly to call it so, thus avoiding a word which the systematists have all along applied to the caulicle. Although initial stem and initial root are most clearly discriminated in the present volume, yet, in the accounts of the germination of the ordinary Dicotyledons, it appears to be implied or stated, either that it is the root-part which first projects from the seed-coats and that the stem-part begins its development later, or that the axial part of the embryo conspicuously preëxisting in the seed is root and not stem. We take it to be quite otherwise, namely, that this axial part in the seed is cauline, and that ordinarily it protrudes or makes some growth in length before root-formation begins.

A few misprints of names of plants will in nowise mislead or trouble any botanist, except possibly in the case of *Apium graveolens*, which on p. 422 and 424, and in the index, is printed *Apios*. A. G.

2. *Eucalyptographia: A Descriptive Atlas of the Eucalypts of Australia and the adjoining islands;* by Baron Ferd. von Müller, K.C.M.G. London and Melbourne: 1880.—This is the sixth decade of the Atlas, and contains descriptions of the following species: *Eucalyptus buprestium*, *globulus*, *megacarpa*, *miniata*, *occidentalis*, *peltata*, *punctata*, *setosa*, *stellulata* and *tetragona*. The detailed account of *Eucalyptus globulus*, the ordinary "Blue Gum-tree," contains many facts of interest, which may be here briefly noticed.

(1.) Degree of resistance to frost. This depends on the age of the plant (older trees standing best), on the amount of moisture in its surroundings (dry places most favorable), and on the degree of shelter from the wind. Grown-up trees did not suffer at all during the cold winter of 1879–80 at Antibes, although the temperature fell as low as 15° F., and the monks of Tre Fontane, after repeated observations, maintain that the tree will bear a temperature of 17° F., and this appears to be in accord with experience in parts of Australia. In the cool, elevated but sheltered region, surrounding the alpine height of Mt. Buller, Baron von Müller observed snow lodging in large masses, and for protracted periods, on the branches of the tree, eventually injuring even strong limbs, but the stem and main branches remained unhurt, pushing forth new shoots and foliage in the spring. Professor Göppert, Dr. Raveret-Wattel and others "observed that *E. globulus* will bear a severe degree of cold transiently (about 20° F.), if it lasted not sufficiently long to congeal the sap to any great extent and provided also, that the new wood was well matured and the spot of growth a dry one."

(2.) The drainage of swamps by Eucalyptus. According to Baron von Müller, it was through the Archbishop of Melbourne, that plantations of this tree were first established for diminishing the miasmatic exhalations of the Pontine marshes. The following is an extract from a letter from the Archbishop to Baron Müller, under date of 17th Dec., 1879: "The *Eucalyptus globulus* was first raised in the Campagna from seeds kindly presented to me by you on my visit to Rome in 1869, to attend the Vatican General Council. I handed the seeds to the Superior of the Trappist-monks, who occupied the monastery and grounds of the Tre Fontane, a most fever-stricken locality. On my next visit to Rome, made a few years later, I had the pleasure to see the good results of your kind and thoughtful presentation in the vigorous growth of many Gum-trees acting most wholesomely on poisonous air of that portion of the Campagna."

But at Gaëta, trees were planted by Royal order in 1854, and in 1878 one of them measured eleven feet in girth, and was 100 feet high. It may be remembered that another species of this genus, *E. amygdalina*, surpasses this one both in rapidity of growth, and in the height which it ultimately attains.

The author deals to some extent with the medicinal properties of the products of the tree, and has also some remarks upon the influence of the forests of Blue-gum trees upon phthisis.

(3.) Strength of the timber. In one set of experiments the pieces subjected to test were seven feet long by two inches square.

No. 4,	seasoned	8 months,	broke with a total weight of			819 lbs.
No. 6,	"	2 to 3 years,	"	"	"	1029 "
No. 15,	"	20 years,	"	"	"	1330 "

In some experiments with wood of the same dimensions, Mr. Laskett found that when the weight was suspended in the middle, both ends free, the average was 712 lbs., being very much less

than that recorded by Mr. Mitchell. Experiments by Baron von Müller and J. H. Lühmann, upon wood two feet long and two inches square, gave the following results:—Weight required to break a truncheon of

Eucalyptus globulus,	2252 to 3752 lbs.
Eucalyptus Leucoxylon, Victorian Iron Bark Tree,	3144 "
Eucalyptus obliqua, "Messmate,"	1776 "
Quercus alba, American White Oak,	2086 "
" " "	1644 "
Pinus silvestris, Baltic Deal,	1358 "

The vertical or crushing strain on cubes of two inches was from ten to twelve tons.

So much for the strength of this wonderful wood. Now one word regarding the yield of wood by a single tree. At South-Port, Mr. James Dickinson noticed a tree of *E. globulus*, which, according to a local shipwright, would fully suffice to build a ninety-ton schooner. In addition to the figures of the species and the usual analytic details, the present decade contains a well-executed plate showing the histology of the bark of Blue-Gum, and a full bibliography of the same species. The latter well bears out the statement of the author, that "Perhaps not even to the Royal Oak of England has such an extensive literature been devoted at any particular period as to our Blue-Gum tree within the last twenty years." G. L. G.

3. *The Flora of Essex County, Mass.;* by JOHN ROBINSON. Salem, Essex Inst., 1880.—This is a substantial octavo of 200 pages. It is not a systematic description of the vegetation of the district, as might perhaps be inferred from the title, but it is far more than a mere catalogue. It comprises a list of the flowering plants and a large part of the Cryptogams, with copious notes upon the localities, where such information seemed necessary. Mr. Robinson's valuable annotations deal with a wide range of subjects, but never wander out of sight of the plant. A short and discriminating sketch of the early Botanists of Essex County adds greatly to the interest of the volume. G. L. G.

4. *Botany of California*, Vol. 2; by SERENO WATSON. University Press, Cambridge, Mass. 1880.—This volume completes the Flora of the State of California. The first volume has been re-issued with typographical corrections; the second volume containing all the important additions to the earlier orders. A notice of this very attractive and most useful work will be given in a subsequent number of this Journal. The volumes can be procured from the Curator of the Herbarium of Harvard University, Cambridge, Mass. The price to botanists is $5.00 for each volume, (postage 45 cts. additional). G. L. G.

5. *Giant Squid* (*Architeuthis*) *abundant in* 1875, *at the Grand Banks;* by A. E. VERRILL.—From Capt. J. W. Collins, now of the U. S. Fish Commission, I learn that in Oct., 1875, an unusual number of giant squids were found floating at the surface, on the Grand Banks, and mostly entirely dead, and more or less mutila-

ted by birds and fishes. In very few cases they were not quite dead, but entirely disabled. These were seen chiefly between N. lat. 44° and 44° 30′; and between W. long. 49° 30′ and 49° 50′. He believes that between twenty-five and thirty specimens were secured by the fleet from Gloucester, Mass., and that as many more were probably obtained by the vessels from other places. They were cut up and used as bait for cod-fish. For this use they are of considerable value to the fishermen. Capt. Collins was at that time in command of the schooner 'Howard,' which secured five of these giant squids. These were mostly from ten to fifteen feet long, not including the arms, and averaged about eighteen inches in diameter. The arms were almost always mutilated. The portion that was left was usually three to four feet long and, at the base, about as large as a man's thigh.

One specimen, when cut up, was packed into a large hogshead tub, having a capacity of about seventy-five gallons, which it filled. This tub was known to hold 700 pounds of cod-fish. The gravity of the *Architeuthis* is probably about the same as that of the fish. This would indicate more nearly the actual weight of one of these creatures than any of the mere estimates that have been made, which are usually much too great. Allowing for the parts of the arms that had been destroyed, this specimen would, perhaps, have weighed nearly 1,000 pounds.

Among the numerous other vessels that were fortunate in securing this kind of bait, Capt. Collins mentions the following: The schr. "Sarah P. Ayer," Capt. Oakly, took one or two. The "E. R. Nickerson," Capt. McDonald, secured one that had its arms and was not entirely dead, so that it was harpooned. Its tentacular arms were thirty-six feet long. The schr. "Tragabigzanda," Capt. Mallory, secured three in one afternoon. These were eight to twelve feet long, not including the arms. These statements are confirmed by other fishermen, some of whom state that the "big squids" were also common, during the same season, at the "Flemish Cap," a bank situated some distance northeast from the Grand Banks.

The cause of so great a mortality among these great Cephalopods can only be conjectured. It may have been due to some disease epidemic among them, or to an unusual prevalence of deadly parasites or other enemies. It is worth while, however, to recall the fact that these were observed at about the same time, in autumn, when most of the specimens have been found cast ashore at Newfoundland, in different years. This season may, perhaps, be just subsequent to their season for reproduction, when they would be so much weakened as to be more easily overpowered by parasites, disease, or other unfavorable conditions.

IV. Miscellaneous Scientific Intelligence.

1. *Zeitschrift für Instrumentenkunde: Organ für Mittheilungen aus dem gesammten Gebiete der wissenschaftlichen Technik.* No. 1, January, 1881. 40 pp. small 4to. Berlin (Julius Springer—B. Westermann & Co., agents in New York.)—The prospectus of this new Journal states that it will be devoted to the discussion of all subjects immediately connected with the design and construction of scientific instruments and apparatus. These will be treated both from the standpoint of the investigator and also that of the mechanician, in order that the scientific learning of the former and the technical knowledge and experience of the latter may be combined to produce the best results. The list of editors comprises twenty names, including many of those who have the best reputation in Germany for the manufacture of scientific instruments, as well as able physicists. In the highest class of research in all branches of physical science, and too in their practical applications, the use of instruments of precision takes so prominent a place that every effort that is made with the object of improving them cannot fail to have an immediate useful effect. This the new Journal promises to do, and the names of those who appear in the editorial staff, and of those from whom articles are promised, in early numbers, are a guarantee that its standard will be a high one.

The January number contains the following articles:—Normal barometer and manometer, by R. Fuess; on the illumination of micrometer arrangements, by W. Förster; on the construction and investigation of micrometer screws, by C. Reichel; on spectrum apparatus, by H. C. Vogel; a rotating spectrum apparatus, by O. Lohse; on the graphic method in Physiology and on a telegraphic kymometer, by H. Kronecker. In addition, there are also short notices, reports of society transactions, and abstracts from periodical and patent literature. In the numbers next to follow are promised articles on the present stand of thermometry, the construction of balances of precision, the application of electricity to meteorological registration apparatus, and many others of equal interest.

2. *Academy of Sciences, Paris.*—Dr. B. A. Gould, of Boston, who has been for many years Director of the Observatory at Cordoba, and has published within the past year the Uranometria Argentina, as a first part of the results of his telescopic study of the Southern heavens, has been elected a member of the Section of Astronomy of the French Academy of Sciences, in place of the late Professor C. A. F. Peters, of Kiel.

3. *Bibliographie Générale de l'Astronomie.* Tome II. *Mémoires et Notices insérés dans les Collections académiques et les Revues.* 1st fasciculus; by J. C. Houzeau and A. Lancaster, Brussells, Dec. 1880. Large 8vo, pp. 86, and 336 columns.—This is the first part of the second volume of a general Bibliography

of Astronomy, the first volume not yet having appeared. The first eighty-six pages are devoted to lists of the Academic collections and journals, giving the leading bibliographic details of the several series. The rest of this number contains two sections, *History and Study of Astronomy* and *Biographies of Astronomers*, also the first part of Section 3d, Spherical Astronomy. The second section, containing references to the biographical literature for apparently about 2,500 men more or less connected with the progress of astronomy, is of peculiar value and appears to be very complete. The work, as a whole, will be one of the highest value. H. A. N.

4. *Washburn Observatory, University of Wisconsin.*—Professor Henry S. Holden, of the Naval Observatory, Washington, has received the appointment of Director of the Washburn Observatory, in place of Professor Watson, deceased.

5. *A Text Book of Elementary Mechanics for the use of Colleges and Schools;* by EDWARD S. DANA. 291 pp. 12mo, with 190 wood cuts. New York, 1881. (John Wiley & Sons).—The character of this elementary text book will appear from the following quotation from the preface: "The chief aim has been to present the fundamental principles of the subject in logical order and in as clear, simple and concise a form as possible, yet without any sacrifice of strict accuracy. For the sake of making the portions of the subject, which necessarily involve some difficulty, more intelligible to beginners, and also to increase the interest of the general principles demonstrated by showing something of their practical bearing, simple illustrations have been introduced rather more fully than usual; these are sometimes given in a few words, sometimes in more extended form," etc. The first fifty pages are devoted to the subject of Kinematics; then follow several chapters on the laws of motion, central forces, work and energy, etc., embraced under Dynamics or Kinetics, and the remainder is devoted to Statics. The relations involved in the various simple machines are developed both from statical principles and too in accordance with the principles of work. A large number of examples are given at the close of each section, and a supplementary list of examples involving the metric units is added at the end of the volume.

6. *Massachusetts Institute of Technology—Abstract of the Proceedings of the Society of Arts for the 18th-year*, 1879–80; meetings 242–255 inclusive. 108 pp. 8vo. Boston: 1880.—This volume contains a record of the meetings of the Society of Arts from Oct. 23d, 1879, to May 13th, 1880, including abstracts of the various papers presented; the subjects of some of these are: the parabolic reflecting camera, by W. H. Clarke; an instrument for the determination of latitude, by S. C. Chandler, Jr.; artificial alizarine, by Professor J. M. Ordway; battery and copper plate amalgamation, by Professor R. H. Richards; American interoceanic ship transit, by Dr. S. Kneeland, etc.